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MARGARET FLOY WASHBURN (1871–1939)

Miss Washburn is typical of what is best in American psychology. Her activity covered the span of the truly modern movement. She studied with Cattell in his first year at Columbia when strictly experimental psychology was just beginning to receive recognition. Her later work was with Titchener, who inspired a different approach, one that was central in the discussions of the early generations. Her publications covered many of the more important fields in American work. Beginning with sensation and perception she quickly developed an interest in animal psychology. *The Animal Mind* was the first comprehensive survey of experimental work in animal psychology. She participated in the discussion of most of the theoretical questions that were current through the growth of schools and developed a system of her own that was comprehensive and attracted much favorable attention. It was original in formulation and grew out of a thorough knowledge of the literature.

Miss Washburn was born in New York City, July 25, 1871. Her father's family was predominantly of American Quaker stock. Her maternal grandfather was from Devonshire. Her early education was by private tutors with attendance later at the public schools. She entered Vassar College at sixteen. Aside from the required courses she was most interested as an undergraduate in chemistry and philosophy. On the basis of her college training she was attracted to philosophy and science as a field of further work. She chose to devote herself to psychology as a subject that involved something of both, and sought admission to the graduate school at Columbia to further this purpose. At that time women were not admitted, but Professor Butler advised her

to apply to Cattell for admission as a hearer and she was accepted. She at once took full part in the seminary and experimental work. In addition she was encouraged to begin a piece of experimental work on Weber's Law as applied to the limen of twoness. Obviously she was accepted as a participant rather than as a hearer.

On Cattell's recommendation Miss Washburn became Scholar at Cornell the following year, the first year of Titchener's period at Cornell. Her thesis topic was an outgrowth of the study she had begun at Columbia and took the title 'The Effect of Visualization upon Judgments of Tactual Distance and Direction.' When completed it was translated into German with Titchener's aid and published in Wundt's *Philosophische Studien.* At Cornell she was interested in philosophy as well as in psychology and was perhaps as much influenced by the men in philosophy as by Titchener who was at the time the only psychologist on the staff. The two fields were much closer at that time and the contacts between students and members of the staff in the two subjects at Cornell were especially frequent.

The writer remembers Miss Washburn well from the Cornell period. He served as observer for her work on the skin and was led to a study of a related character probably as a result of the work and her suggestions. She was a brilliant conversationalist, inclined to be rather acid in her comments on men and things. Her keen sense of humor was fully developed at this time. To a shy youth impressed by her two years seniority as a student and one year in chronological age, she seemed somewhat reserved and aloof. As I look back I can see no specific occasion for the old opinion and it vanished completely as I came in contact with her in later years.

Miss Washburn's attitude towards Titchener and his theories offers some phases of interest. At first there was a close sympathy between them in point of view, possibly because neither had as yet developed the position that was later regarded as characteristic. Titchener had not formulated his structuralism at that time, although he followed Wundt's general course in his theorizing. His main aim was to estab-

lish psychology as a science. His main reaction was against the religious notion of a soul entity that still held over among psychologists with a theological background. Structuralism as a closed system developed later under the spur of criticism from the functionalists and others. It was the outgrowth of his defense of the implications of some of his earlier statements against the attacks of these critics.

When the more rigid system developed, Miss Washburn showed a lack of sympathy with the more extreme tenets. In several articles and in seminar discussion she indicated her scepticism of his formulation of the relation between sensation and attribute and also of the exclusive use of introspection as the method of psychology. In her autobiography she remarks: "It is worth while to describe conscious states, but not, in describing them, to turn them into something unrecognizable." They never came to a break but Miss Washburn published several articles which showed a divergence in the point of view. Personally also in her later period at Cornell she came closer to the men in philosophy than to Titchener. This may be seen in the estimate she gives of Titchener in her life of him in the *Dictionary of American Biography* which I should say was eminently fair but not enthusiastic.

After receiving her degree Miss Washburn accepted the professorship of philosophy at Wells College which she held for six years. During this period she spent regularly a day a week at Ithaca, using the library and taking part on occasion in seminar discussions. Following this for two years she was Warden of Sage College, the residence hall for women at Cornell. The second of these years she was also a lecturer in psychology, giving courses in animal psychology and in social psychology. During the eight years that succeeded bestowal of the doctorate, she was occupied more or less with research at Cornell. An important problem was a study of the flight of colors under the controlled conditions afforded by the physics laboratory. This period was a highly formative one in her general training.

After a year as assistant professor of psychology at the University of Cincinnati, Miss Washburn was given

the rank of associate professor of philosophy at Vassar, the institution with which she was identified until her death in 1939. Her title was changed to Professor of Psychology in 1908, a chair which she held until her retirement in 1937. From this time on she was a prominent figure in American psychology. At Vassar she was an inspiring teacher in the best sense of the word. The courses were always popular with the students, quite as much for the solid character of the content as for the temporary interest. She took nothing for granted even in related sciences, but taught as much of them as was necessary for an understanding of the use she would make of them. Many students were drawn to the courses and sufficiently impressed with the subject to make it their life work.

An important part of the undergraduate training was the investigation of some small problem within the range of ability of the student, that might afford an initiation into research. In many cases these proved worthy of publication and were printed as one of the *Minor Studies from Vassar College*. Sixty-seven of these were published over the years in the *American Journal of Psychology*. They constitute probably the longest series of studies from any of the American laboratories. All were worth while and many were valuable contributions.

Miss Washburn's contributions to the literature of psychology were many. Possibly the best known is *The Animal Mind*, which was first published in 1907 and was thoroughly revised three times—in 1917, 1926, and in 1936. It was an admirable compilation of the experimental work that had been done in animal psychology and contained many original suggestions for organization and interpretation. Through it one can follow the changes in theory that developed from the different schools and from the changing character of the experimental work. The first edition was the earliest attempt to collect and organize the scattered work in the field. In addition to the compilation and organization of the material, important contributions were made in the interpretation of results. Now and again a suggestion would be made that

later was published by an experimental worker without recognizing its source. Miss Washburn once remarked that it was too bad to publish a new result in a general work only, for it would probably be overlooked, or if noticed, it was likely to be regarded as part of the general literature and not as the contribution of the author of the book.

It is interesting to note some of the changes that were made in the different editions. The first devoted a good deal of space in the introductory chapters to a review of the results and position of the early anecdotal school in animal psychology and opposed to it the advantages of the experimental method. In the later editions less space was given to this material but it was not entirely omitted from the last edition, long after psychologists in general had ceased to think of anecdote as a real method. The nonprofessional reader still needed the warning.

A second point of general interest was the rise and fall in the emphasis placed upon mental states in the interpretation of animal behavior. In spite of the growing scepticism in Titchener's structuralism, Miss Washburn always spoke of animal responses in terms of human consciousness. This emphasis is seen most clearly in the fact that she devoted the second chapter in each edition to a discussion of the criteria for the existence of mind as consciousness. It was admitted that consciousness could be demonstrated only in man. Still arguments were arrayed as to what functions and what structures might be accepted as evidence of the existence of consciousness. The essentially negative conclusion was unchanged in the later editions. It was nevertheless repeated that consciousness must be assumed in the higher animals and must be lacking in the lower and that none of the criteria were sufficient to determine at what point mind or consciousness first appeared.

The omission and reinstatement of chapters show the influence of the dominant schools. In the second edition, published in 1917 when the behaviorist movement was at its height, several pages are devoted to the behaviorist's position. After the discussion, the explicit statement is made that "we

shall be interested in what animals do only as it throws light upon what they feel." Nevertheless that edition omitted the chapter on 'The Memory Idea' that had been contained in the first one. Much of the material of the chapter was included in a second chapter under the general head of learning by experience, but the omission of the chapter title may be significant.

In the fourth edition this subjective material was again made part of a new chapter on the 'Higher Mental Processes' which gave much space to Köhler's work and other studies that were developed under its influence. Miss Washburn rejoiced in the rise of the Gestalt school that again made mental processes an important factor in animal psychology. The fourth edition also introduced another new chapter, The Affective Factors in the Animal Mind, Drives, Incentives, and Emotions. Some of the material of the chapter had been discussed elsewhere, but the necessity for a new chapter indicated the great number of experiments that had been devoted to the drive, as well as the importance the term had taken in general psychological theory.

Undoubtedly the *Leitmotiv* of Miss Washburn's theoretical thinking was the importance of motor processes on conscious processes. It creeps out in the discussion of the animal mind where the burden of the argument demonstrates the existence of consciousness in animals. In her autobiography she traces the beginning of her interest in that point of view to about 1901. The first explicit formulation of the theory was in a contribution to the Hall *Festschrift* in 1903. In that article she maintained that the feeling of sympathy was dependent upon the excitation of slight movements which, if they were executed in full, would help the sufferer. The kinæsthetic sensations from these slight movements were the basis of the feeling we call sympathy. Between 1909 and 1914 three papers were written that advanced the theory in different ways. The position was fully formulated and justified in 'Movement and Mental Imagery' published in the Vassar Semicentennial Series in 1916. One of the last papers was a study of the participation of motor processes in the perception

of depth by the aid of double images read before the National Academy of Sciences.

The importance of motor processes in consciousness had been recognized and formulated in different ways by Ribot, Stricker and many others. Miss Washburn related her theories to Münsterberg's presentation in his *Grundzüge* of which she spoke with approval in her autobiography. Nevertheless she opposed her theory to his in one major point. Münsterberg held that consciousness accompanies the free discharge of motor neurones, and the greater the discharge, the more intense the consciousness. When it was pointed out to him that habitual movements were unconscious or only slightly conscious, he asserted in rejoinder that habitual movements were transferred to subcortical centers, and that only the cortical motor cells were accompanied by consciousness. Miss Washburn recognized that this could not easily be brought into harmony with known neurological facts, and opposed to it her own theory that consciousness arose only when a movement or tendency to movement was partly inhibited by a tendency to make another movement. After comparing her statement that consciousness accompanies inhibited motor discharges with Münsterberg's statement, she reaches a compromise in the statement that "consciousness accompanies a certain ratio of excitation to inhibition in a motor discharge, and that if the amount of excitation either sinks below a certain minimum or rises above a certain maximum, consciousness is lessened." [1]

This principle was applied to each of the main groups of psychological phenomena. Perception depends upon the movements that are evoked by the presented object. Perception involves possession of a movable sense organ that can organize an object into a reversible series of sensations. Perception also implies a movable grasping organ that can take an object apart and respond to each portion separately. To appreciate the object as a whole, it is necessary to make movements to the whole as opposed to the parts. In this way

[1] *Movement and mental imagery*, Boston and New York: Houghton, Mifflin, 1916, p. 25.

"Motor psychology can explain the facts of perception which the configurationist merely describes," to quote from the *Psychologies of 1930* (p. 88). Similarly one part of a visual field becomes 'figure' and the rest 'ground' if it seems easier to pick up than the rest.

"The very same process which, on our hypothesis, when added to the effect of an outside stimulus makes that effect conscious, will, when it occurs in a sensory centre that is not being externally excited, be accompanied by the type of consciousness that we call 'centrally excited,' the consciousness that occurs in mental images and thoughts. Whenever a motor pathway is at the same time excited by a sensory pathway and partially inhibited by an antagonistic motor excitation, a process occurs in all sensory pathways connected with the motor pathway by low synaptic resistances, including the sensory pathway that is exciting the motor pathway in question. This process is accompanied by consciousness." [2] Thus memories as well as perceptions were connected with the activity of motor paths. Similarly associative revival depends upon the sensori-motor connections and not upon sensori-sensory paths. Thus "each movement arouses kinæsthetic sensations, these then excite the second motor center." During any delay that occurs in the discharge of this center, the sensory center that belongs to it is centrally excited, and there occurs an image in consciousness. A chain of ideas is at basis a chain of movements.

The more complicated mental processes are given an explanation upon the same basis. The *Aufgabe* or problem idea that looms so large after the work of Külpe and Ach finds its physiological and truly causal explanation in the movements of a tentative character which persist during the pursuit of an end. These correspond apparently to the posture of a dog as he watches at the hole of a coyote. There is tension but little movement. Attention depends on the closely related action systems, or more definitely of the inhibitions of action systems that precede the presentation of the stimulus. The increased consciousness that is the core of attention is

[2] *Ibid.*, p. 30.

due to the inhibitions that must be present in the appropriate degree. Many of the detailed facts of learning and forgetting, among them the effects of distributed as compared with accumulated repetitions, the different forms of inhibition, the differences between primary memories and memory images are explained ingeniously by slight modifications of the general theory.

The phenomena associated with reasoning also were made to fall neatly within the rubrics of the motor theory. A concept, *e.g.*, is the accompaniment of a single response to a number of stimuli. The single response groups all the members of the class into a unity, and the consciousness that accompanies it is the general idea. A very interesting and plausible explanation is given of the imageless thought processes that were emphasized by the Würzburg School and by Woodworth and Binet. These were related to partially excited movement systems. Each stiuation evokes its own tentative or partial movement. These give each its own kinæsthetic sensations and these sensations in turn constitute the basis of the certainty that accompanies an intellectual state without specific images. Thus 'but' is represented by two incompatible movement systems which by their mutual inhibitions prevent movement but give a mass of kinæsthesis that constitutes the 'but' consciousness.

Dissociation also is explained by incompatible movement systems one of which represses the other. The importance of emotions in producing dissociation and the suppression of complexes is ascribed to the highly complex masses of movements that accompany or constitute the emotions. The motor systems that are not compatible with those developed by the emotion are inhibited during the emotion, and the conscious processes connected with the motor systems are repressed with them. Explanation of the more lasting forms of dissociation of personality follows the same lines.

On rereading *Movement and Mental Imagery* the writer was impressed anew with the erudition of the writer, the wide range of literature that was taken into account, and the skill with which it was incorporated in the main outline of the argu-

ment. It is the work of a master of the detailed facts of psychology and an artist in exposition. It is too early to pass a final judgment on the truth of the conclusions, but the system certainly stands high among the many attempts to bring all the known facts into a unitary statement and to formulate an explanation of them.

The wide-spread character of Miss Washburn's interests is shown clearly in the great variety of topics that were covered in the *Minor Studies*. In the early years many of them deal with the ways feeling and emotional processes affect memory and perceptual processes. In the later years different forms of animal experimentation have a prominent place. Some of the studies bear upon the motor theory but on the whole the theory plays a minor rôle. Various tests, and perception and sensation were all well represented. Most of the topics treated were important. All the papers show great care in the execution of the experiments, and ingenuity in the interpretation of the results.

The mass of Miss Washburn's published writings is impressive. Including the Vassar Studies more than 130 articles came from her pen and approximately seventy reviews. *The Animal Mind* had four editions and was translated into Japanese. *Movement and Mental Imagery* was the major theoretical work. In addition she translated Wundt's *Ethical Systems* and her thesis was published separately as a book of sixty pages. As further evidence of her wide participation in the life of psychology, she was associate or cooperating editor at one time or another on six of the periodical journals. Her closest connection was with the *American Journal of Psychology*. She had long been cooperating editor and when Titchener resigned the editorship in 1925, she with Bentley and Boring shared the editorial work with Dallenbach.

Miss Washburn had received most of the honors that can be given in recognition of psychological achievement. She was one of the two women to be elected president of the American Psychological Association. She was President in 1921; Miss Calkins had been President in 1905. She was a member of the National Academy of Science, also one of two women

members, Dr. Sabin being the other. In 1926–27 she was vice-president of the American Association for the Advancement of Science and chairman of Section I (psychology). She was also a member of the Society of Experimental Psychologists and of the International Committee of the International Congress of Psychology. An honor quite unique was the presentation to her of a commemorative volume of the *American Journal of Psychology*. It is a work of nearly 450 pages, containing contributions from thirty-two American psychologists and men of related fields. The volume was prepared and presented to her at the Columbus meeting of the Association in 1927.

Miss Washburn had an important place in the social and official life of psychology. She was frequently called upon to function on committees of the Association and in these capacities always showed great initiative, good taste, and tact. She was a faithful attendant at the meetings of psychological groups and had an unusually wide circle of friends. Her death is a great loss to psychology. She contributed much personally and professionally, and represented a unique point of view. All will regret the loss of a sympathetic, always genial and witty companion and a sane advisor on matters theoretical and practical.

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REFLECTIONS UPON THE NATURE OF THE PSYCHOLOGICAL SELF

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A real dichotomy, within the world of 'common sense,' is that between the self and its surroundings. This dichotomy implies fundamental postulates concerning the nature of reality, on the one hand, and the nature of knowledge, on the other—postulates which are inherent not only in descriptions by authors of fiction, of poetry, or of drama, but even in the simplest descriptive statement made by one man to another, such as the statement: "My cigarette lies smoking on the ash-tray." While the layman—even the author—may be completely ignorant of and unconcerned with these implications, the philosopher or the psychologist may be interested in trying to extract them. To the present writer this is an essential problem, for it constitutes the necessary introduction to any survey of the nature of the psychological self. The more important of these presuppositions will, therefore, be briefly described. As will be noted, they all concern either the nature of reality or being (the ontological problem), or the nature of knowledge (the epistemological problem). In turn, the epistemological postulates concern knowledge of environment, on the one hand, and knowledge of self, on the other.

(1) A fundamental postulate is the proposition that there exists a world around us (our environment) which is real, not—as Berkeley wanted it—a mere idea in our minds. Each individual's environment—in the broadest sense of the term—is the total universe minus himself, that is to say, it includes all other living beings as well as all inanimate things, possibly more. By implication, this postulate asserts a second proposition, namely, that there exist selves each one of which is capable of being surrounded; each one of which, in order to

be surroundable, must have its own unequivocal boundary. The determination of the boundary of a self has to be left to a later page. Each self is a constituent of the world around every other self. *Ex necessitate rei*, this fact includes it among realities.

(2) An equally fundamental postulate is that, whenever we experience, we are aware at least of certain aspects of this world and that, in a measure, our apprehension is adequate; in other words, that it symbolizes or stands for real aspects of the universe. Except for some measure of such correspondence our experiencing would not be able adequately to guide our conduct in, our adaptation to, our environment, as it actually does. It is possible, even likely, that only a few aspects of the universe are symbolized in experience. Select features of experience have been elaborated into scientific constructs and systems of constructs; no one single system of scientific constructs, however refined, is likely to express the universe *in toto*.

(3) The second postulate, asserting that we experience aspects of the real world and that this experience is relatively valid, implies that men possess common practical knowledge. This necessitates a third important premise, namely, that the symbols in the terms of which one individual is aware of the world around him resemble, in a certain measure, the ones in the terms of which other people experience the same aspect of the same real world. Except for such resemblance, it would be impossible to understand, for instance, how joint effort on the part of members of a group could take place, or how social hierarchies regulated by the aid of language could have arisen.

As has been stated earlier, the reality of each self as a bounded entity is a corollary of postulate (1). Postulate (2) asserted that certain aspects of the world around us are experienced in terms or symbols which sufficiently correspond with the real nature of these aspects to be of aid in adaptation. Among the experienced items of the world around us are our fellowmen. From this follows that each self—being an apprehendable item of the world around other people—is apprehendable by them. But, it does not follow, as well,

that each self is capable of being experienced by itself; for no self is an item of the world around itself. Consequently, if a self is aware of itself, this case is not covered by postulate (2), referring merely to the knowability of aspects of the world around us.

Supposing, nevertheless, that a self is aware of itself and, furthermore, that its experience of itself symbolizes its real nature sufficiently well to aid its adaptations; this case, again, is not covered by postulate (2). A self's real nature, in the foregoing sentence, would be its self as—according to postulate (1)—it actually exists in the environment of other people.

It follows from postulate (3) that each one of us, being aware of one and the same of our fellow-men, will apprehend him in terms which in some measure resemble one another. But it does not follow that an individual's experience of himself resembles other people's experience of him, for a self is not outside itself.

If we assume, nevertheless, that a self's experience of itself sufficiently resembles other people's experience of it to aid cooperative endeavor, then this case is not covered by postulate (3), referring merely to a resemblance between men's apprehension of environment.

The supposition of the previous paragraph does not concern resemblance between different selves' experience of their own respective selves. If we suppose that, in a general way, a self's experience of its own self and other selves' experience of their own respective selves sufficiently resemble one another to aid cooperative endeavor, then this case is covered neither by the anteceding supposition nor by postulate (3).

A consideration of the previous statements makes it evident that the second and third of the postulates so far formulated—while they refer to experience of environment—do not cover experience of self.

Unsophisticated description of events of every-day life imply definite postulates concerning the nature and validity of experience of self; postulates which, I believe, may be briefly formulated as follows:

(4) Just as we are aware of certain aspects of the world around us (postulate 2), we are also aware of certain aspects of ourselves. Just as our experience of the world around us symbolizes or stands for real aspects of this world, representing these sufficiently adequately to aid adaptation (postulate 2), so does our self-awareness symbolize real aspects of our selves with sufficient adequacy to aid adaptation.

(5) Just as the symbols in the terms of which one individual is aware of the world around him resemble, in a certain measure, the ones in the terms of which other people are aware of the same aspect of the same real world (postulate 3), so do the symbols in the terms of which one individual experiences himself sufficiently resemble the ones in the terms of which other people are aware of him to make possible and to aid cooperative endeavor.

(6) Just as one individual's experience of himself, in some measure, resembles other people's experience of him (postulate 5) so do the symbols in the terms of which one individual is aware of his own self resemble the symbols in the terms of which other people are aware of their own respective selves, this resemblance being sufficient to make possible and to aid cooperative endeavor.

Fundamentally, these six premises are implied in the common practical thinking of most laymen. As a consequence, any one literary description in terms implying these premises will be paronymous with and accessible to common sense understanding.

The most remote implication of common sense thinking and accordingly of the six postulations outlined has not, as yet, been drawn. It can well be done by starting from postulate (5).

It would seem evident that the resemblances referred to in postulates (5) and (6), though in a measure valid, are very general. This has probably several reasons which, to begin with, we need to discuss only with reference to the resemblance posited in postulate (5), the resemblance of one man's experience of himself with other people's experience of him.

One of the reasons why this resemblance can be only

general lies in the ambiguity of any strict demarcation of the boundary of a self. Let us, for the sake of analysis, take the simplest of all views concerning the boundary of the self, the biological, and consider a human being to be his behaving, bodily organism. This view is simple, is based upon abstraction, and is scientific. No normal human being, in his practical contacts with his fellowmen, apprehends these as mere bodily organisms each one bounded by its own respective cuticle. Still less does a normal human being apprehend himself merely or essentially in this manner. Whenever he experiences himself as spatially extended, it turns out that his awareness of that aspect of his self differs on different occasions, presenting, at one time, peculiar expansions, at another time contractions. It should be noted that such fluctuations do not occur in other people's experience of him as extended. On the contrary, his fellowmen's experience of him is fairly constant. In other words: Whenever A apprehends B (another person) as spatially extended, B is a constant while, whenever A apprehends A (himself), in this manner, A is a variable. Expansion and contraction of the extended self is a subjective, not an objective phenomenon.

There seems to be a certain lawfulness in the fluctuations of a person's experience of himself as spatially extended. In illustrating this lawfulness, I shall, as a matter of convenience, denote the biological organism as (O), the experienced expansion of the organism to include parts of the environment as the (O+)-phenomenon, and the experienced contraction of the organism to exclude parts of itself as the (O−)-phenomenon.

It seems as if, during activity, the experienced, or subjective, extended self tends to expand and to include the tools or implements which it applies and controls. This (O+)-phenomenon seems to be definitely more conspicuous whenever the employment of the implement requires effort on the part of the self. But that relation is valid only within limits. On the one hand, whenever the effort required by the application of the tool becomes so intense that it interferes with the effective control of the latter, the (O+)-phenomenon

disappears while, on the other hand, whenever the effort required by the application of the tool is only slight, the (O+)-phenomenon is only indistinct. If you use a crowbar to move a heavy stone, your extended self—as you experience it—expands to include the crowbar, the world around you beginning at the place where the weight of the stone challenges your effort. Similar effect occurs, I believe, not only when we use manual tools—like the crowbar—but also when we use receptive tools—such as, for instance, a microscope—the application of which requires a certain amount of effort. If your tools are clumsy and remain ineffective in spite of very strong effort on your part to control them, the (O+)-phenomenon breaks down. For instance, you throw down the too-heavy crowbar and curse it. In some measure the (O+)-phenomenon occurs even when your effort in applying an implement is slight. This seems to be indicated by a variety of modes of speech. For instance, you say: "I turned on the light." What you actually did was to turn a switch while the current liberated produced the light. Nevertheless your report of your act suggests the expansion of your subjective self to include a set of auxiliaries which have been under your control. A similar example would be when, after having used the telephone, you say "I called up New York." If, in any one of these cases, you should fail to obtain the effect you desire, that is to say, in the first case, if you should fail to produce light by turning the switch, or, in the second case, if you should fail to reach New York in spite of repeated attempts, then, again, the (O+)-phenomenon disappears.[1] Whenever an auxiliary is completely self-regulating, that is to say, when no effort on your part is required in order to make it pursue its function, the (O+)-phenomenon seems to be negligible. It does not apply, for instance, to the automatically regulated air-conditioning system in your house, though this system is an auxiliary in the service of your desired comfort.

[1] Cmp. A. D. Ritchie, *The natural history of mind*, New York: Longmans Green Co., 1936, pp. 198–201. Mr. Ritchie quotes William James as having given examples of the (O+)-phenomenon. Cmp. also Ernst Mach, *Die Analyse der Empfindungen*, Jena: Gustav Fischer, 1918, p. 10 ff.

I have illustrated what I have called the (O+)-phenomenon, the expansion, under certain circumstances, of the subjective extended self to include parts of what, in the biological sense, is environment. Instances of the (O+)-phenomenon could be easily multiplied. Let us now turn to the opposite, the (O−)-phenomenon, namely, the contraction, under certain conditions, of the subjective extended self to exclude parts of the biological organism.

The (O−)-phenomena fall into two groups or types: (*a*) loss of a part with projection, and (*b*) loss of a part without projection. Here follow a few examples of the former type.

Suppose that you recline in a chair, your legs stretched out in front of you within your field of vision. Suppose, further, that a little kitten is playing with your shoe-strings and that you passively watch his antics with a slight amusement. In this attitude of repose you might well experience the whole scene before you, including not only the kitten but your feet and part of your legs as well, as outside you—your own limbs belonging no longer to you but to the world around you. A second interesting instance. Once, dozing peacefully in a hammock, my eyes half closed, I suddenly became slightly frightened by a black object in front of me. A moment later, I discovered that the object was my own foot. In numerous instances when you simply compare, in some respect or other, a visible part of your own body with an object outside you, as for example, when you try to ascertain whether the moon looks larger or smaller than your hand, you are very likely to project your own limb into the environment, to experience it as part of the same.

In all these instances of (O−)-phenomena, the characteristic condition under which they seem to occur is comparative bodily relaxation. This is true not only of the first two instances but, also, of the comparison case. For, as soon as the holding of your hand against the sky produces fatigue and experience of bodily effort, the (O−)-phenomenon disappears and the hand is again absorbed into your subjective bodily self.

Functional paralyses of members of the body, as they

occur in certain cases of hysteria and as they can be induced by hypnotic suggestion, may properly be considered as (O−)-phenomena of type (*b*). The hysterical patient or the hypnotic subject does not feel the member as real. Its belongingness to the subjective extended self seems, in a way, to be lost. The condition of the disorder, according to the theory to which I adhere, is a complete inability to direct effective effort into the control of the member. As (O−)-phenomena, the functional paralyses would be instances of type (*b*), because the lost member was not projected into the environment.

The fact that bodily relaxation seems to be the condition under which the (O−)-phenomenon takes place makes it interesting to compare this phenomenon with the total loss of awareness of one's own extended self that sometimes occurs during waking relaxation. Such instances, involving no projection of the lost biological organism into the environment, are—like the paralysis—(O−)-phenomena of type (*b*). Perhaps some of the readers will recognize the following experience. You are reclining in perfect comfort, eyes closed. You are not asleep; you may be day-dreaming or passively reminiscing. You lose all awareness of your own extended self to such a degree that, when suddenly you desire to move, for a moment you are unable—as it were—to localize your own members. As a consequence you cannot control them. The paralysis is only transitory and is rapidly resolved into normal bodily control.

In connection with the (O−)-phenomenon, it is of interest to call attention to the fact that our autonomic physiological systems which, normally, function without felt effort on our part hardly ever are experienced as parts of our subjective extended self.[2] The fact referred to—being an exclusion from the subjective extended self of parts of the biological organism—might well be regarded as an (O−)-phenomenon; insofar as the excluded parts are not projected into the environment, the phenomenon would be of type (*b*). Its occurrence seems

[2] Breathing, whenever obstructed—that is to say, whenever its continued function requires effort—might become an aspect of the subjective extended self.

to conform with the general law manifested in the case of the total loss of awareness of one's extended self as well as in the case of the functional paralysis, namely: no felt effort applied to total or partial bodily control, no awareness of the body or of parts of the body. It is a singular parallel that one's internal autonomic systems as well as one's external self-regulating auxiliaries should not become incorporated into one's subjective extended self.

So far, we have discussed deviations of the subjective extended self from the boundary of the biological organism as these deviations are manifested in the (O+)- and the (O−)-phenomenon. Postulate (5) asserted that the symbols in the terms of which one individual apprehends himself—in our present connection his extended self—sufficiently resemble the ones in the terms of which other people apprehend him, as extended, in order to make possible and to aid cooperation. If this postulate is valid, it follows that there must be a prevailing number of occasions when a person's subjective extended self and his extended self as experienced by others—we will call it his objective extended self—approach congruence. When is this? As a person's objective extended self remains relatively constant, our inquiry really concerns the determination of the conditions under which his subjective extended self conforms with his objective extended self. The reply to this question is very simple. Whenever a person is overtly active and feels his own effort being applied to effective control of his own bodily members and organs his subjective extended self approximates his objective extended self.

Though our experience of ourselves as spatially extended is vague, it reveals, as we have seen, certain interesting lawful features.

It is of the greatest interest that the condition upon which the boundary of the subjective extended self depends, when conforming with the objective extended self, is the same as the one upon which the boundary of the expanded subjective self depends; and, in the negative sense, the boundary of the contracted subjective self, notably, felt effort effectively

applied to the control of means to an end. In the case of the expanded subjective self, or the (O+)- phenomenon, the felt effort is effectively applied to the control of auxiliaries extraneous to the organism, whereas, in the case when the subjective and the objective extended selves conform, the felt effort is effectively applied to the control of bodily members and organs. In this connection, it is also of interest to recall that completely self-regulating extraneous auxiliaries, the functioning of which requires no application of effort on our part (our automatically regulated air-conditioning system), as well as completely self-regulating internal auxiliaries, the functioning of which is also pursued apart from felt effort (our autonomic physiological systems), are not apprehended as parts of the subjective extended self.

The similarities which I have indicated in the previous paragraph might lead one to consider the members and the organs of the body, in a certain sense, the auxiliaries of a self that is more *fundamental* than the subjective extended self; the expansion of this basic self, whether to include the body alone or the body plus extraneous auxiliaries, would then be of the same nature as the expansion we have described as the (O+)-phenomenon, and the condition of the expansion would be identical with the condition of the latter phenomenon, that is to say, felt effort applied to effective control of means to an end. Such consideration raises a very important question: What would be the nature of this fundamental self? A reply requires the analysis of certain aspects of the subjective self which are far more significant than its extension.

No doubt, by far the most important aspect of the self is the striving self, the self that pursues goals, realizes ambitions, consecrates effort to the fulfillment of ideals. Every person is aware of himself as a striving being; this aspect of his self we will call his subjective striving self. His fellow-men, also, are aware of him as a striving being, the latter to be denoted as his objective striving self. The resemblance between subjective and objective striving selves implied by postulate (5) is, as a rule, only general; however, a select number of one's fellow-men—those with whom one is in continual

intimate contact—might learn to know one's striving self with sufficient accuracy and completeness to make them able to predict one's conduct. This knowledge might be called their knowledge of one's character; character, as McDougall defines it, being that which "gives stability, constancy, harmony, predictability to conduct; that in man which makes him trustworthy, dependable." [3] One's character, as recognized by others, is apprehended by oneself in the terms of consistency or endurance of one's striving, and unity of one's dominating ambition or ideal. Inherent in one's experience of the permanence and unity of one's own endeavor is, by necessity, one's memories.

The enduring unity of one's subjective striving self might break down during pathological conditions. The cases of break-down, as observed by medical psychologists, support the proposition that the most fundamental aspect of subjective self-hood is the unity of striving. On this point McDougall writes:

> . . . the core of personal identity, the foundation of our belief in our own reality and continuity, is the experience of purposive striving . . . of putting forth effort . . . of resolving and acting, of pursuing a goal; which experience is fundamental and unique. . . . This immediate awareness . . . together with memory, the memory of our past strivings, is the ground of our belief in our reality as enduring entities.
>
> What is the evidence for this view? . . . we may invoke the evidence of pathology. In cases of dual or alternating personality of the fully developed kind, we always find two changes: first, a discontinuity of memory; secondly, some radical alteration of the conative tendencies. The changed or secondary personality may have knowledge of some of the past experience of the normal personality, but does not remember it as his own experience; he knows it merely as he may know of yours or mine. And his system of impulses and desires is quite another. He has a distinct system of sentiments or conative trends. He desires and strives to achieve other goals.

[3] W. McDougall, *The energies of men*, New York: Scribners, 1933, p. 186.

> It is, in fact, the rise of some system of desire, incompatible with the system that constitutes the core and essence of the normal personality, that leads to the conflict which issues in dual personality. . . .
>
> Pathology illustrates our point in another way. There are patients who become afflicted with a 'sense of unreality.' Such a patient feels that nothing is real, not even himself. This seems to be due . . . to lack of desire, of impulse, of conative energy. Through exhaustion (perhaps by reason of internal conflict) the patient's conative energies are sapped; he no longer responds with strong impulses or desires, and all things become unreal. . . .
>
> . . . Descartes' famous dictum . . . needs revision; not 'I think, therefore I am' but 'I strive, therefore I exist,' is the foundation of all belief. . . .[4]

My earlier survey of the subjective extended self prompts me to some further reflection upon the three types of pathological cases referred to in the passage quoted: (1) the cases of alternating personality with amnesia, (2) the cases of alternating personality without amnesia, and (3) the cases of feeling of unreality.

In all cases of alternating personality there seems to have occurred a splitting of the normal subjective striving self, a splitting accompanied by a loss of reciprocal balance or control of the parts. One of the disintegrated parts is experienced by the patient as his own striving self, the others may not be recognized at all, or they may be experienced as projected, that is to say, as independent objective striving selves. The former is the case whenever reciprocal amnesia accompanies the alternations, the latter whenever no such amnesia is present. Any one alternating self equals the normal subjective striving self minus the other alternating selves. In other words, any one alternation represents a contracted subjective striving self. This contraction I shall call the (S−)-phenomenon, indicating, in this way, its parallelism with the (O−)-phenomenon, that is to say, the contraction of the subjective extended self. As we shall soon see, the (S−)-phenomena—like the (O−)-phenomena—might

[4] W. McDougall, *Outline of psychology*, New York: Scribners, 1923, pp. 370–375.

be of an (*a*) or of a (*b*) type, namely, they might or they might not involve projection.

In alternating personality with reciprocal amnesia (an (S−)-phenomenon of type (*b*)) each phase of alternation, namely each subjective striving self, is unaware of the others. Consequently, no projection can appear. The import of these cases, as well as of the ones without amnesia, would seem to be that systems of impulses that cannot be controlled might be lost as aspects of the subjective striving self. The (S−)-phenomenon in the amnesic cases is closely parallel with the pathological form of the (O−)-phenomenon, the functionally paralysed member. It might even be spoken of as a paralysis of a member of the subjective striving self.

The cases of alternating personality which do not involve amnesia differ in certain respects from the amnesic ones. Here, in a manner, each self is aware of the others, but it is not aware of them as subtractions from the normal subjective striving self. It knows them as projected, actually experiencing them as other striving selves, or, objective striving selves. There are two features of particular interest in these cases:

(1) The loss and projection would seem to constitute a parallel—in the sphere of the subjective striving self—to the (O−)-phenomenon, type (*a*), in the sphere of the subjective extended self. The conditions of the two phenomena would seem to be similar though not identical, the (O−)-phenomenon being conditioned by a complete absence of the application of effort to the lost part, the (S−)-phenomenon by a complete loss of ability to strive toward, or to exert effort toward the attainment of certain ends.

(2) The projection is in no measure a projection in the spatial sense. For, the patient, though he experiences his alternating selves as objective striving selves, maintains that these, whenever they are dominating, occupy his own body. A negative and a positive implication are inherent in this particular feature of the case: (*a*) the subjective extended self is of relatively little significance in the experience of subjective self-hood, and (*b*) the experience of subjective self-hood is paramountly dependent upon striving.

In cases of feeling of unreality, there appears a loss of realness not only of the patient's subjective striving and subjective extended selves but, also, of the totality of his environment—including all his fellow-men. Feeling of unreality is conditioned by a general lowering of the level of intensity of the patient's conative life, due mostly to exhaustion by mental conflict. The phenomenon presents a relative parallelism with one of the (O−)-phenomena of type (*b*), notably, the total loss of awareness of one's own extended self during complete repose in the waking state.

The general significance of the reflections submitted lies in the fact that they force into prominence felt striving as the ulterior generator of our experience of subjective self-hood; the consideration of the feeling of unreality cases being of particular additional interest in that they suggest, not only the dependence of our experience of subjective self-hood upon striving, but, also, the dependence of all experience of the world around us upon conation.

The last mentioned dependence should, I believe, be brought into relation with an occurrence of every-day life known to everybody, namely, the fact that, whenever we strive towards a certain well-defined end, our striving seems to exert a selective activity upon our experiencing at the time. More especially, we are essentially aware only of our goal and of things related to it, that is to say, on the one hand, of means to the end desired, and, on the other hand, of obstacles obstructing our achievement. This well-known phenomenon furnishes another illustration of the dependence of our experience of the world around us upon our mode of striving at the time,—the same dependence which was generally indicated by the feeling of unreality cases.

In discussing the (S−)-phenomenon, I have begun with a survey of pathological cases. This is because these cases offer very conspicuous illustrations of the phenomenon. There are, however, many occurrences in every-day life which indicate that, in a less pronounced degree, the (S−)-phenomenon is fully normal. For instance, we all have moments of weakened self-control, during which we might commit

acts that later we regret. At such times, our retrospection upon our acts brings them before us as strangely unreal; but, not only do our acts seem unreal, we ourselves also seemed unreal as we committed them. How likely we are to exclaim: "I was not myself when I did it!" It seems to us as if the impulse that had prompted our deed is not a part of our subjective striving self. I see no reason why we should not consider instances of this kind at least incipient (S−)-phenomena of type (*b*).

Whenever we refer to contraction of the subjective striving self, we refer to the contraction of a magnitude that is not spatial but, rather, of the nature of significance or import. The same would be true when we could refer to expansion of the subjective striving self. Is there, in this sense, an expansion of the subjective striving self; in other words, is there an (S+)-phenomenon as well as an (S−)-phenomenon? If so, what are the characteristic conditions of the expansion of the subjective striving self?

As the preparation for a brief inquiry into this problem, let us recall that the core of our character as apprehended by ourselves is the consistency and endurance of our striving and the unity of our dominating ambition or ideal. Let us also recall that lacking unity of strong urge leads to conflict which is likely to result in a contraction of the subjective striving self, sometimes in pathological alternation of personality. From these data we might logically infer the conditions of the expansion of the subjective striving self. But we can go further than a logical deduction of the nature of the latter. Its nature is evident from our common experience.

Whenever a number of people coöperate in genuine endeavor to further one and the same end, each one exerting the best of his effort in its accomplishment—whenever this is the case, the subjective striving self of any one member is experienced as expanded, as incorporating the striving selves of all the collaborators. Accordingly, in such instances of coöperation, each subjective striving self approaches congruence with any one objective striving self; from which it follows, also, that each subjective striving self presents the

highest possible degree of congruence with each subjective striving self in the group. We have, in other words, the highest possible degree of resemblance posited by postulates (5) and (6). To such an active union of men, the much discredited concept of 'group mind' would, if anywhere, apply.

Summary

(1) A distinction has been made between the extended self and the striving self, and a further distinction between subjective and objective extended self, on the one hand, and subjective and objective striving self, on the other hand. The term subjective self has been used to denote one's self as apprehended by oneself, the term objective self to denote one's self as apprehended by other people.

(2) While a person's objective extended self is relatively constant, his subjective extended self might differ from time to time; at times expanding (the (O+)-phenomenon), at times contracting, (the (O−)-phenomenon type (*a*) or (*b*)).

(3) In some obscure manner, the variations of the subjective extended self seem to be a function of two conditions: (*a*) felt effort exerted in the control of certain means to an end, (*b*) relative bodily relaxation. More especially, the (O+)-phenomenon would seem to be conditioned by (*a*), the (O−)-phenomenon by (*b*).

(4) Whenever a person applies felt effort to effective control of an objectively extraneous auxiliary, his subjective self tends to expand and include that auxiliary (the crowbar, the light-switch, and the telephone case).

(5) Whenever a person is in a state of relative bodily repose, his subjective extended self does not seem to be as firmly knit as when he is bodily active. On occasions he might even experience parts of his own body as belonging to the world around him rather than to himself (the kitten-shoe-lace, the fright, and the comparison case). This is the (O−)-phenomenon of type (*a*), including projection.

(6) During deep waking repose, a person might entirely lose awareness of his extended self (the transitory paralysis case). This is the (O−)-phenomenon of type (*b*), including

no projection. Another (O−)-phenomenon of the same type is, probably, the functional paralysis of bodily members.

(7) Whenever a person is overtly active and feels his own effort being effectively applied to the control of his own bodily members and organs, his subjective extended self approximates his objective extended self.

(8) A person's subjective striving self—like his subjective extended self—might differ from time to time, at times expanding (the (S+)-phenomenon), at times contracting (the (S−)-phenomenon of type (*a*) or (*b*)).

(9) Continued conflict might result in complete inhibition of all ability to strive toward certain goals. Such inhibition might well be regarded as a contraction of the normal subjective striving self, that is to say, as an (S−)-phenomenon.

(10) The part of the subjective striving self which is lost through contraction may be projected, that is to say, it may be experienced as an objective striving self, occupying, at times, one's own bodily organism. This would be an (S−)-phenomenon of type (*a*) and would be the case in alternating personalities without amnesia.

(11) The part of the subjective striving self which is lost through contraction may not be projected. This would be an (S−)-phenomenon of type (*b*) and would be the case in alternating personalities with amnesia.

(12) Expansion of the subjective striving self (the (S+)-phenomenon) occurs in each member of a group who genuinely coöperates with the rest in the pursuit of one and the same goal. Under such condition, accordingly, each subjective striving self reaches the highest possible congruence with all other subjective striving selves in the group.

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ASSOCIATION AND THE LAW OF EFFECT

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Common speech defines acts in terms of their results, not in terms of the movements by which those results are accomplished. We eat a dinner, sail a boat, ride a horse, play a selection on the piano. For each of these acts there may be a thousand different patterns of muscular contraction in the details of the achievement, and the act may still be known by the same name. Skillful and awkward performances of the same act may use very different motions.

The notion of conditioning, or a behavioristic use of the principle of association by contiguity, applies directly to the prediction of movements and not to the prediction of acts as defined by their results. There are no direct neural connections between any sense organs and our dinner, our boat, or our piano. It is not the changes in these environmental objects that are associated with new cues. Associative learning implies sense organs, afferent pathways, a central nervous system, a motor system, and the final term, the effector. In the strict sense it is the activity of effectors that is predicted, not the results of this activity in the outer world.

The fact remains that in order to apply a strict associationism to the explanation of human and animal behavior we must deal eventually with acts as well as with movements. There is little use for the prediction of movements alone, and there exists almost no vocabulary for their description. To try to describe behavior by naming the muscles in use and the degrees and order of their contraction would be absurd. It is the changes brought about by movements, changes usually in the environment and not in the organism, that are of practical importance, and theories of behavior must somehow bring acts as well as movements into their predictive laws and principles.

All this has been said many times before and by many men, perhaps by no one so explicitly as by the late A. P. Weiss. It is this difficulty in extending association from movement to significant action that is behind much of the current dissatisfaction with the principle of association or conditioning. The Pavlovian experiments and their successors stand apart from the bulk of studies of learning. Pavlov dealt with the secretion of a gland without reference to the possible effects of that secretion on the life adjustments of the dog. Similarly those experiments that have used reflex winking to a blast of air, reflex flexion of the leg to a shock, reflex changes in breathing induced by shock, and all the rest have paid little attention to the effects of these responses on the animal itself or on the outer world.

Contrasted with these experiments in association proper are the experiments in maze learning, puzzle-box solving, discrimination, rote learning, improvement in skill. In these experiments in learning proper the object of interest is act, not movement. And these studies have paid, until recently, scant attention to association. They center about the fact that acts undergo changes that can be predicted from their effects. Interest lies in the effects of effects,—the effects on later action of the effects of a prior action, however achieved. Only in the last few years has it also been pointed out that the conditioning experiment itself appears to depend on the effects of the action, and not merely on the pairing of original and substitute stimuli.

It is the search for an act psychology that accounts for the current tendency to distinguish two kinds of association, and for the mistaken notion that there is some sort of opposition between the concepts of association and of the law of effect. Many psychologists have expressed the belief that if the law of effect is valid it must represent an exception to the principle of association. There are no *a priori* grounds for this belief. The principle of gravitation is not taken to be abrogated in cases in which it does not predict or give the full significance of an event. Man is assumed to be a gravitating object even though he may have in the meantime solved a

problem, begotten a child, or entered on death or sleep. The principle of gravitation does not mention these eventualities. A man may gravitate and still be a good Baptist. Even in his baptism he is assumed to remain faithful to the principle of Archimedes.

Among the recent authors who have suggested that there are two kinds of conditioning are Hilgard, Skinner, and Razran. There are a number of others, notably Culler, who would reformulate the principle of conditioning to have it predict not movement but action. We may examine some of these opinions.

In 1937 Hilgard [1] proposed recognizing two varieties of conditioning. His words were:

> The behavior of the conditioned response superficially resembles the operation of the principle of effect. . . . There is an important gap, however, between the reinforcement of a conditioned response as ordinarily conceived, and the rewarding of a cycle of behavior. In conditioning situations the reinforcing agent intensifies the behavior already under way by itself evoking similar behavior; in ordinary learning situations the reward intensifies an activity sequence by evoking behavior which may be quite dissimilar. Thus the salivation of the dogs to the bell is increased by following the bell with food and the tendency to open the latch of a problem-box is also intensified by food. In the latter case, however, the food does not evoke behavior which resembles latch-opening. It is a mistake to gloss over this difference, lest one of the most important barriers to a conditioning theory of learning be ignored instead of overcome.
>
> The importance of considering this difference between the reinforcing of behavior with an agent which produces similar behavior, and rewarding it with an agent which produces heterogeneous behavior, cannot be over-emphasized. . . .

We may, he says, take any one of three positions on this problem. (1) We may recognize two distinct classes or kinds

[1] E. R. Hilgard. The relationship between the conditioned response and conventional learning experiments. *Psychol. Bull.*, 1937, **34**, pp. 73.

of learning, reinforcement-extinction and reward-and-punishment. (2) We may undertake to demonstrate that the operation of reward is reducible to the operation of reinforcement. (3) We may recognize two kinds of conditioning which differ in the responses measured rather than in the responses that occur. Hilgard's preference is for the third position. My own is for the second. His shrewd qualification that the two kinds of conditioning *differ in the responses measured* leaves these two positions ultimately reconcilable.

From the use made of 'reward' conditioning by Hilgard it is evident that this indicates acts rather than movements, while 'reinforcement' conditioning deals with a movement or a secretion following an unambiguous 'unconditioned' stimulus.

Skinner's recent thoughtful book, *The Behavior of Organisms*, uses two classes of conditioning also. The first of these is the conditioning of the familiar Pavlovian experiment, and this he calls Type S. It is defined thus:

> The approximately simultaneous presentation of two stimuli, one of which (the 'reinforcing' stimulus) belongs to a reflex existing at the moment at some strength, may produce an increase in the strength of a third reflex composed of the response of the reinforcing reflex and the other stimulus.

Skinner's second type of conditioning requires the definition of an 'operant reflex' as:

> . . . an identifiable part of behavior of which it may be said, not that no stimulus can be found that will elicit it . . . but that no correlated stimulus can be detected upon occasions when it is observed to occur. It is studied as an event appearing spontaneously with a given frequency.[2]

The law of conditioning of the second type, Type R, is then: "If the occurrence of an operant is followed by presentation of a reinforcing stimulus, the strength is increased." Skinner's distinction between his two types is based, like Hilgard's,

[2] B. F. Skinner. *The behavior of organisms.* New York: D. Appleton-Century Co., 1938, p. 18.

on the differences in the laboratory methods of recording reinforcement and reward experiments.

In a recent number of the PSYCHOLOGICAL REVIEW Razran joins Hilgard and Skinner in recognizing two classes of associative process. In Razran's opinion certain results indicate that we must recognize "a special higher form of conditioning that may best be called qualitative conditioning, in which a quality, a direction, a tendency, an effect—a vector or a valence, if you prefer—is the chief conditioning datum." [3] This appears to be a somewhat unclear statement to the effect that qualitative conditioning is the conditioning of an act. Razran finds Pavlovian or 'quantitative' conditioning is like qualitative conditioning in that: (1) both result from contiguity; (2) functionally new connections are formed between stimuli of antecedent S–Rs and the responses of subsequent S–Rs in both sorts; and (3) repeated withholding of the conditioning stimulus weakens the connection.

The differences between the two are: (1) that subsequent S–Rs in quantitative conditioning invariably weaken their antecedent S–Rs, while the subsequent S–Rs in qualitative conditioning can strengthen, weaken or reverse their antecedent S–Rs; (2) extinction, or repeated withholding of the stimulus of the subsequent S–R, restores the original response of the antecedent S–R in quantitative conditioning but reverses the antecedent S–R in qualitative conditioning; and (3) "the modification phenomena of the second type [quantitative conditioning] are common characteristics of any successful linkage between S–Rs, but the specific modifications of the first case [qualitative conditioning] are evidently confined to only a special group of subsequent and antecedent S–Rs." [4] These last 'subsequent S–Rs,' in case the reader has not recognized them, are rewards and punishments, which have special effects according to their natures. Razran does not find anything like qualitative conditioning in autonomic responses or in simple conditioning.

[3] G. H. S. Razran. The law of effect or the law of qualitative conditioning. PSYCHOL. REV., 1939, 46, p. 445.

[4] *Ibid.*, p. 449.

Razran further explains that in qualitative conditioning we find the conditioned interaction of organismic qualities, sets, tendencies, directions, etc. Quantitative conditioning denotes specific, segmental, self-contained conditioning. Again I find that this is a rather unclear approach to saying that the one deals with acts, the other with movements.

Culler finds a tendency for his conditioned response to start out to be a repetition of the practiced unconditioned response in all its detail (movement) but that it changes as practice is continued and becomes eventually some adaptive preparation for the original stimulus. This also I take to mean that what starts out as a conditioned movement develops into a conditioned act.

> The localized, individuated CR of a fully trained dog (foot-withdrawal just before the shock) is as unlike his early yelpings and jerkings as two acts well can be. . . . The CR, in brief, is nature's way of getting ready for an important stimulus.[5]

It is my own belief that we can abstract a single principle from the results of the experiments on conditioning and that the experimental results are in general quite consistent with that principle. *When a movement is executed, certain of the stimulus patterns active at the time tend to become cues for that movement.* This appears to me to underlie and to be the essential feature of all learning. To say that this is offered as the sole explanation of learning is, of course, absurd.

Such a statement of the principle includes, obviously, some distressing ambiguities; of these I have written elsewhere. Both movement and situation are hard to describe. Behavior is a continuous flow that does not readily yield nameable features, and does not offer exact repetition in detail. The analysis of behavior and its antecedents will always be rough and approximate. At the same time it is only by means of such analysis that we can predict or understand behavior, because prediction and understanding depend on nameable and observable antecedents and these are used for the pre-

[5] E. A. Culler. Recent advances in some concepts of conditioning. PSYCHOL. REV., 1938, 45, p. 136.

dicting of nameable and observable consequents. These are, moreover, always parts and not wholes.

With statements like that made by Allport in his recent book on *Personality* I find myself puzzled. Under the caption 'Learning,' which he encloses in sceptical quotation marks, he writes as follows:

> A more liberal interpretation of the law of effect is offered by the doctrine of *Insight*, whereby human beings are credited with the ability to survey and organize the confused field that confronts them, intelligently comprehending the potential relationship of the various factors to one another, and finally adopting the course of behavior most suitable to their needs. By virtue of this ability one line of conduct is understood to be more suitable than another and is accordingly adopted. (It must be assumed, of course, that what every individual is doing is to strive always for a more effective relationship between himself and his behavioral environment.) Even though insight may *fail* in its attempts to change the modes of adjustment or even though it may render them *less* suitable, still the very existence of insightful behavior presupposes that intelligent effort and evaluation underlie the phenomena of change in personality.[6]

In all of these opinions, Hilgard's, Razran's, Skinner's, Culler's, and Allport's, the difficulty faced is that men and animals obviously solve problems, achieve success, do the 'right' thing. The fact that these very authors are now alive is, when we consider the hardships and dangers that must have been overcome by their long lines of ancestors, evidence that man is an adaptive animal. Our very existence at this late date in the history of our species indicates a human capacity for insight, resourcefulness, cunning, that these men find not provided for in simple associative learning. 'Mere' association does not, on the face of it, explain these qualities.

Faced with this difficulty, Allport has taken the most radical stand. His explanation is simply that man is a bright animal. Error is acknowledged to occur. "Insight may

[6] Pp. 154–155.

fail," but its failure is just that. The result of learning, when learning occurs, is success. Error is accidental and not lawful. The law of insight is to the effect that men can be trusted to face confusion and finally to adopt the course of action most suitable to their needs. 'Blind' conditioning, 'mere' association, does occur, but it is of no interest to Allport since "it does not do justice to the many selective functions of the human mind that have the power of limiting and directing the course of conditioning." It may rule in limbo but not in heaven.

Culler, who is responsible for some of the most ingenious and careful experimentation in conditioning, takes a line much like Allport's. The first UR is a readiness for something:

> But if this improved UR were his only recourse, the animal would still be forced to wait in every case for the stimulus to arrive before beginning to meet it. The veil of the future would hang just before his eyes. Nature began long ago to push back the veil. Foresight proved to possess high survival-value, and conditioning is the means by which foresight was achieved.[7]

Hilgard's suggestions I find less clear. His 'reward-conditioning' which produces 'heterogeneous behavior' instead of the 'similar' behavior produced by reinforcement is nowhere stated as a principle or law. He appears to assume that what he calls heterogeneous behavior is actually the appropriate or right behavior that Culler predicts. But prediction can go further than that the animal will do 'something appropriate.' We can predict in terms of 'mere' association just what the animal will do, just what one of many appropriate movements will be performed. It is true that this prediction requires close observation of past behavior, but it is nevertheless specific and useful.

Skinner has attained a high degree of objectivity by confining his description to items on his record. But I suspect that he has hidden in his notion of 'spontaneity' much the same natural gift for doing the right thing that Allport commends so highly. The assumption made by most writers,

[7] *Ibid.*, pp. 135–136.

says Skinner, that all behavior is occasioned by stimuli is quite unjustified.

> There is a large body of behavior that does not seem to be *elicited*, in the sense in which a cinder in the eye elicits closure of the lid. . . . Merely to assert that there *must* be eliciting stimuli is an unsatisfactory appeal to ignorance.[8]

Certain behavior appears to be "emitted" by the organism, and Skinner insures that it will continue to *appear* to be emitted by putting the animal in a closed box where the operator will not be tempted to conjecture the possible eliciting stimuli. And it is this 'emitted' behavior that appears to get results for the animal and to be fixed by subsequent reward or possible success. The 'emitted' act in most of his experiments consists in depressing a bar.

Razran's qualitative conditioning is also his explanation for success. It is the attachment to a cue not of a definite movement but of a 'quality, a direction, a tendency, an affect, a vector or a valence.' It 'has a number of characteristics not predictable or derivable from known facts of quantitative or mere linkage conditioning.' It constitutes the supplement of 'blind' association or 'mere' association and accounts for doing the right thing at the right time.

To these attempts to describe learning so as to include the obvious facts of 'blind' association and the *equally obvious fact* that men achieve success and improve their skills could be added many others. Warner [9] found that he had conditioned not a definite movement, but a general tendency to escape by one means or another. Liddell, James and Anderson [10] say that their sheep are conditioned "to do something appropriate to the situation."

I have as high an opinion of my fellow men as has Allport. I have also a hearty respect for the apes whose clever behavior has been described by Yerkes and Köhler. I find that many

[8] *Ibid.*, p. 19.

[9] L. H. Warner. An experimental search for the 'conditioned response.' *J. genet. Psychol.*, 1932, **41**, 91–115.

[10] H. S. Liddell, W. T. James, and O. D. Anderson. The comparative physiology of the conditioned motor reflex; based on experiments with the pig, dog, sheep, goat and rabbit. *Comp. Psychol. Monogr.*, 1934, **11**, p. 89.

of the lower forms of animal life exhibit insight in that they somehow learn to do the right thing or one of the many possible right things in a difficult situation. But my interest is in how they do this. And I am not content with the simple statement that they somehow do, even though this is set down as a psychological principle. I have no objection to the obviously true statement that men and animals succeed and improve. But I object seriously to principles that pretend to detail *how* these fine results are achieved and then merely assert that they are.

Furthermore, to hold that one type of learning operates in success and another in failure appeals to me as highly unreasonable. The structure of the nervous system remains the same throughout success and failure. Success is an achievement of the whole animal, associative learning and all, and not the result of the occasional working of insight or a success principle or a special goal-oriented type of learning. Allport argues that "the very existence of insightful behavior presupposes that intelligent effort and evaluation underlie the phenomena of change in personality." I would also hold that the wide-spread occurrence of 'mere' association suggests that it underlies 'insightful' behavior as well as behavior that does not manifest insight.

Very few psychologists (Hull, Hovland, the Yale group in general, and Hunter are notable exceptions) have made an effort to discover the possible extent of associative phenomena, or to examine the possibility that 'mere' association is involved in all learning. The interest of most men lies in the fact of success and not in the examination of how that success is achieved. There are too few studies in which the actual details of the animal's movements are noted, studies like that reported by Kellogg at the Berkeley meeting of the Psychological Association. Of the thousands of rat-maze experiments no single one, so far as I recall, has observed the trial-to-trial progress of individual animals and the development of associative changes in the animal's method. Associative phenomena are excluded when runs are averaged or errors and time are recorded in a curve of learning. Interest and the

record deal with the final achievement, its total time and errors, and not with the progress of the learning by which the final result is brought about.

One important reason why so many experimenters in the field of conditioning (which deals directly with association and not with goals) have been led to desert an objective description for a more loose and easy description in terms of success is that on so many occasions the response to the new signal has not been the movement supposedly associated with the signal but something else, something which may often be loosely called adaptive. Behavior is generally adaptive. But there are many reasons why an associative cue may fail to be followed by its associated movement.

A buzzer is sounded and a guinea-pig in a wheel-cage is given a shock through the contact of its feet with the metal grid on which it stands. The pig runs. But after practice of this sequence for many trials the response to the buzzer becomes not running but crouching. The associative cue appears to elicit behavior that was not originally associated with it and could not have been predicted in terms of mere association.

To conclude that this is an exception to the rule of association by contiguity is to act hastily. We have in many experiments noted that a response to a continuous signal like the buzzer tends to be set forward in time. This is quite understandable. The buzzer is qualitatively constant, and the response associated with the final period of its sound might well be called out by earlier phases. If the pig is thus caused to start running *before* the shock is applied, the training situation is distinctly altered. The pig is now shocked *while running*, and my own guess is that the response to being shocked while running is not running but crouching. Thus we have, unwittingly, substituted a new 'unconditioned response,' namely, crouching. And the fact that the pig now responds to the buzzer by crouching is easily understood.

Too many experimenters have followed the lead of Pavlov and observed an extremely limited set of features of stimulus situation and of response. Very few of them ask the question

that must be asked if we are interested in association: *What was the animal actually doing when the cue was given?* What, for instance, does Culler's dog do when the shock is received while the leg is flexed? To assume that shock has only one response attached to it is absurd. Response is as much determined by the animal's prevailing action and posture as it is by extraneous stimuli like shock. If one dog receives a thoracic shock only when it fails to lift its leg in time and another dog receives thoracic shock whether it does or does not lift its leg, these two dogs are being given very different training. What does a dog with leg lifted do when it receives a thoracic shock? It is quite possible that in these experiments the signal is in all cases regularly followed by the behavior that was induced by the accompanying situation, and that the conditions of the experiment shifted that behavior to something else during the experiment.

Ask a class of students to take a coin and be prepared to tap the desk promptly with it when you move your hand. Instead of moving your hand, say "Boo." The whole class will usually respond with the prepared response, though this was never associated with the signal. Wickens' subjects [11] were conditioned to remove the fingers from a metal plate on receiving a signal that had accompanied a shock. They then turned over the hand and it was discovered that they flexed the fingers at the signal instead of extending as in the conditioning practice. This is not to be wondered at, nor is it a valid reason for rejecting the notion of associated movement. Without any training series whatever most persons, informed that they are about to receive a shock through the metal plate, will make whatever movement is necessary to remove the hand *as it lies* from the contact. This not only indicates that they are intelligent and have insight, but also that they have had varied experiences in getting the hand away from danger. The insight is the result of the experiences, not in itself the cause of the action. They had previously acquired a capacity for an act.

[11] D. D. Wickens. The transference of conditioned excitation and conditioned inhibition from one muscle group to the antagonistic muscle group. *J. exper. Psychol.*, 1938, **22**, 101–123.

My suspicion has come to be aroused by all such terms as 'dynamic,' 'organismic,' 'directive,' 'vector,' and not because these terms are either unnecessary or invalid. Any complete psychology must deal eventually with acts and achievement as well as with movement. The objection is not to these terms or to what they signify. It is to the fact that they are almost always used to excuse a failure to undertake the only type of painstaking analysis that will arrive at the 'how' of learning. Such analyses do not make thenselves, and the failure of a man who prefers the easier road of direct 'success' prediction does not indicate that a more industrious and curious psychologist would not have succeeded. When, for instance, a Lewinian description in terms of directed tensions is applied to the fact that children forget completed tasks more thoroughly than interrupted tasks, we should not be content with this as a final generalization. We should ask: What is there about the completion of a task that interferes with the recall of that task? And we should demand the answer in terms of observable features of the behavior, not in terms of unobservable tensions. It is possible that the difference between completing a task and being interrupted at the task involves an active 'turning from' the task in the former case, and that this behavior establishes 'turning from' as a new association with the existing cues. Interruption of a task may fail to induce forgetting by such alienating cues. The difference between completion and interruption would then seem to be that in completion the task itself offers cues for turning away while in the interruption of a task new stimuli break up the current activity. It is also possible that in inducing a child to break off a task the child is caused to verbalize the interrupted task and so fix it in memory through verbal associations. Once a task is completed, the child may turn its verbal attention to the next task.

Can the direction of learning toward success be analyzed in terms of mere association? How do success and reward operate in this direction? Can the obvious ability of the higher animals and of some of the lower to move in one way or another to achieve an end result, to perform an action, be

explained in more elementary terms, or must this ability simply be accepted as a category of behavior? Is action subject to a fundamental description in terms of stimulus, response, and association, much as the evolution of useful structures was reduced by Darwin to terms of inheritance, variation, and natural selection?

I have elsewhere indicated that I believe acts are thus analyzable in terms of movement. I may here summarize the outstanding assumptions that appear necessary to account for the tendency of an animal to learn success or to respond properly to reward and punishment. But first there is required some definition of 'success.'

For the sake of the argument a successful act may be defined as an act that terminates the persisting stimulation responsible for activity and excitement. We might use the phrase 'pseudo-success' for describing certain acts that do not terminate the maintaining stimulation, but, instead, terminate the general activity and excitement. Taking morphine or jumping off a bridge does, in a fashion, offer the solution of the current problem.

Success so defined usually requires some change in the outer world, catching the rabbit, getting the coin in the slot, grasping the object. Even so simple an act as reaching out and grasping something cannot be defined in terms of movement, since it may be excuted from many different positions and at many different distances. Even such grasping as may be done by a one-year-old infant has behind it thousands of episodes in which the sight of something has occasioned behavior. This behavior was at first un-coördinated and awkward. Objects are not at first grasped by an infant until there is an accidental contact with the palm.

Many experiences in which a disturbing object is eventually grasped tend to establish many movements as associated responses to a variety of cues. The reason that grasping is habit-forming is that the disturbing nature of the object changes abruptly when it is in contact with the hand and the last association with the object seen in its disturbing position was reaching out and grasping. All the vague and misdirected

reactions of the baby to the bright rattle tend to be eliminated because they do not remove or materially change the appearance of the rattle. This gives opportunity for them to be supplanted by other movements, and these by others, until some final movement does away with the situation and leaves the situation faithful to its last associated movement. Grasping, biting, clawing, are as truly skills as are billiards and tennis. Acts are defined by success. And if our attention is directed toward the success and we neglect the details of the movement, as is done by most experimenters in learning, we shall fail to discover that all acts are built of movements. Students of learning will do well to notice particularly errors, which are admittedly associative movements, and which prove that the phenomenon of association is still present in the acquisition of a skill. Just what difference between a successful movement and an unsuccessful movement there might be that would class one as an associative response and the other as something else is difficult to see.

Success does not always relax the animal. This is my objection to the new form of the law of effect used by Shaffer in his *Psychology of Adjustment.* Success may bring, and usually does bring, new stimuli which account for new disturbance, and these in their turn become associated with the responses that protect them from new associations by removing them from the situation through changing the orientation of sense organs or through direct action on the stimulating object.

The first of our assumptions will be made tentatively, to the effect that the occasions (not the causes) for movements in an animal are normally stimuli to sense organs. This is the assumption to which Skinner objects. It is justified because it leads in a great many instances to the discovery of effective indicators of behavior to come. On the basis of the animal's posture (which is an entirely proper observation of its proprioceptive stimulus situation) and its movements and its surroundings we may make detailed predictions of the behavior to follow.

A second assumption is the principle of associative learn-

ing, namely, that stimulus patterns active when an act is initiated will serve on later occasions to favor the reinstatement of the act.

We may infer from these two assumptions that when a series of movements is performed, the movement-produced stimuli of one part of the series become associative cues for the next movement of the series. In other words, any series of actions, once performed, tends to be reinstated as a whole if later initiated. This is, of course, subject to interruption and disturbance from the external situation.

From this definition of the nature of success, and from the assumption of the principle of association and its consequence, the tendency for serial behavior to be repetitive, we may expect an animal that is confronted with a disturbing situation to respond in one of the ways that has brought past relief. The details of its movement will depend on its own posture and action which impose strict limits on the effects of disturbing stimuli. A fighting dog cannot be expected to respond normally to its master's voice, no matter how well trained.

Certain types of stimulus situation thus accumulate through association a repertoire of alternative movements which relieve or change the situation. This is what I take Hull to mean by his "habit-family-hierarchy." When we have adequate past observation of the animal and adequate information of the situation (internal and external) in which it is now placed, we can predict the specific movements that will appear. Lacking these items of information, we shall be limited to predicting that the animal will do one of the things that has given past relief.

In a new situation the animal will respond in terms of familiar parts of the situation. This follows from our second assumption, and involves interference in the animal's behavior and the development of new forms of response.

If we now place an animal equipped according to our assumptions (which do not pretend to be complete) in a problem-box or a maze, our prediction of what it will do on the first occasion before the new learning has taken place must be made in terms of past observation of other cats in that

situation, or in terms of this cat's own past behavior in similar situations. If hungry or thirsty or restless when confined in the box, the animal will move actively about, responding to what it encounters in terms of its original nature or training. In the course of this exploration it may eventually perform the movement that releases it or the movement that brings it food.

The effect of the release is obvious in terms of association. Our assumptions will lead us to predict that on the next occasion in the box the cat will go through the same series of movements (not merely the last and successful movement) that it performed on the first occasion. Horton and I find this occasionally approximated with our cats. The reason for expecting this is that the last associations with the box remain the series of movements performed on the first occasion. I use the word 'movement' advisedly, because the repetition is often remarkably detailed.

This prediction of a repetition in movement is complicated by another consideration. If, in its tours of the box, the cat establishes a second association with some repeated situation in the box, and is led (through fatigue or refractoriness or pain or other cause) to respond differently, this will become its new response in that situation and there is evident a short-circuiting in its behavior. With this complication in mind, we predict that the longer the animal remains active in the box, the more will its second trial depart from the first. A short period in the box often means very exact duplication of the movement series on the next occasion.

So long as the animal stays in the box, it is possible for it to establish new associations with what it sees and what it encounters. The final and successful movement tends strongly to be repeated on future trials. Even though the cat operates the mechanism of release it is necessary that the cat notice the open door and leave if the movement of operating the release is to be definitely preserved as a habit. If the cat remains in the box, other movements may displace the 'correct' movement and a new method must be hit upon. Accidental distractions may, of course, break up the long series

of movements leading to success. If we are looking for 'spontaneity' on the part of the cat, there will be numerous instances of it. Muenzinger, whose guinea-pig effected a solution on over one hundred occasions by the same movement, is much more interested in the fact that there was at last a change of movement and a different method began to be used.

The position taken in this paper is that the animal learns to escape with its first escape. This learning is protected from forgetting because the escape removes the animal from the situation which has then no chance to acquire new associations. If the learned movement proves unsuccessful on some occasion, the animal remains in the box and (after possible repetitions as numerous as forty) eventually does something else.

In the few records of maze-learning that I have examined with the above description in mind the facts appear to fit reasonably well. On the first run the rat's behavior must be predicted from our experience with other rats. But whatever blinds are entered on the first occasion tend to be entered on the following occasion, and a blind, when it is at last eliminated, tends to be eliminated for the remainder of the series. We assume that the rat will repeat its first performance, errors and all, except in so far as it establishes new associations in the course of a trial.

In the beginning of this paper Hilgard was quoted to the effect that in the problem-box the food serves to intensify the act of opening the latch, though latch-opening is quite unlike the movements evoked by the food. On this he bases his argument that reward-conditioning must be distinguished from 'mere' conditioning. What I am here urging is that the food reward does not intensify the latch opening. This is the erroneous assumption made by Thorndike in his argument for a law of effect. *What encountering the food does is not to intensify a previous item of behavior but to protect that item from being un-learned.* The whole situation and action of the animal is so changed by the food that the pre-food situation is shielded from new associations. These new associations can not be established in the absence of the box interior, and in

the absence of the behavior of search that preceded latch-opening. The unlearning of a response to a stimulus situation requires the presence of the situation and the forming of new responses in that situation.

If we were determined to apply to reward-learning the law of effect and to disregard the possibility of an analysis in terms of association we should be prevented from making predictions of the style and manner of the act. I do not see in any of the systems of 'goal-psychology' any place for the fact that we can, with the proper information, predict not only the end achievement, but also the details of its execution. The subject will use that one of his practiced movements that was last in evidence when on some former occasion he solved his problem in circumstances like those now prevailing. If there are disturbing new elements in the present situation, trial-and-error behavior will occur and new movements may be added to the repertoire of the act.

In a given individual the capacity for an act, the ability to achieve its defining end result, will depend on previous practice. Repetition under varied circumstances establishes a repertoire of associated movements that will be increasingly certain to be equal to the occasion. But there is no guarantee that the new occasion will not so depart from the practiced ones that no adequate movement is associated. In describing the abilities of a child we are correct in naming certain acts. He can now tie his own shoes, button his own garments (except for unaccustomed arrangements of the buttons), wash his own hands, ride his tricycle. These abilities name aggregates or repertoires of associative movements attached to their proper cues. And when these abilities are once established by adequate practice in varied situations *we may very properly deal with them as acts in associative terms.* Finishing our work (which describes a whole class of situations) becomes the cue for going home (which is also a whole class of movements). When an officer enters the barracks room the soldier 'comes to attention.' Any officer will do and he may enter by any door. The movements of coming to attention will vary considerably depending on the posture in which the soldier is

found. The new recruit who remained lying on his cot and saluted from that position merely lacked training. Here was one posture that had not been associated with any response to officers.

Another illustration: Horton and I noted that our cats that hit upon release by clawing or biting the pole were somewhat more likely to transfer this act to a changed position of the pole. When the successful movement had been to strike the pole with the flank in walking past or to back into the pole, this was not so readily transferred. Clawing and biting at something seen are acts in which the cat is well versed before entering the box for the first time.

One of our large automobile firms has just put on the market a car without a clutch pedal. If their new customers were all to be beginners in the art of driving a car it would be an obvious advantage to place the brake pedal for the left foot which is otherwise left without occupation. But this would equally obviously lead to many accidents in the hands of drivers who are adjusted to the current practice of using the right foot both for brake and for throttle. The company is faced with associative movements rather than a complete act. The development of the act of braking, an act adjusted to any current pedal arrangement, would eventually occur but only after serious accidents caused by the stereotyped form of the existing habits of drivers.

Acts are defined by their goals. Goals are situations which do not break up the associative connections of the movements that brought them about. The goal may or may not furnish the motivating stimuli that lead to activity. The goal itself may be a discovery of trial and error behavior. Gastric distress may make an individual restless. He may eventually hit upon some method of relief. When he does, the sequence of movements, or of acts, that led up to this relief remains associated with the distress. Acts and movements that do not bring relief tend, because of the continuing distress, to be supplanted by new behavior and lose their associative connections with the distress.

So simple an act as sweeping the hearth must have been

established by numerous occasions on which the presence of ashes on the hearth has been associated with action. The original motivating stimuli may have been the scolding of another person. In some fashion or other the individual has been often made uncomfortable when ashes were visible. If the situation were invariable, and the ashes always seen from the same initial position, the hearth broom always hanging in the same place, and the ashes always in the same place, sweeping the hearth would remain the same stereotyped movement that brought original relief. It is because the brake pedal is in the same place from day to day and we are sitting in the same posture that the use of the brake is accomplished by a stereotyped movement. Garage attendants acquire, because they are driving many different types of cars, a repertoire of movements suited to the various mechanisms.

Levin's descriptions place needed emphasis on the dynamism of action. Acts have momentum. But this momentum is subject to analysis. Attention itself is an act that has experience behind it. We have developed the ability to look and listen. Attention to the goal shuts off distracting stimuli by restricting what is seen and heard. By looking at the ashes on the hearth we are kept from responding to our book, our companions, the picture on the wall hanging askew. Attention keeps the action directed to the goal. But the initiation of response to the object attended to also contributes to the momentum of the act. Once we have risen from our chair in response to the sight of the ashes, the movements of rising, the resulting posture, the patterns of muscular tension make reading impossible. We are embarked on a new venture. Severe restrictions on action are made by current posture and action. Even when past experience has not equipped the act with the necessary movement, this momentum of the act maintains the direction of action and allows trial and error behavior to hit upon a new movement solution.

Ashes on the hearth eventually become motivating stimuli in their own right and do not require scolding or other incitement. We learn to maintain many features of the world about us in a certain status because we have been made un-

comfortable whenever they departed from this status. We learn this through reward and punishment, if you will, and the law of effect is a correct if very rough description of the process. But we are at the same time illustrating the operation of association, and in terms of association we are equipped with a much more precise description of the acquisition.

It will be very unfortunate if we yield to the demand for a double theory of learning for we shall then content ourselves with superficial generalizations from experiments with reward and punishment, and cease to look for the details of a basic associative process. To my mind the suggestions that two kinds of learning must be recognized will misdirect future research and burden psychology with descriptive categories that conceal the fundamentals of learning rather than aid in their formulation.

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THE RÔLE OF DEFINITION IN PSYCHOLOGY

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> ". . . the inmost texture of science is logical, and the whole force of any observation made upon the outer world lies in the constancy and mutual relations of the terms it is made in. If down did not mean down and motion motion, Newton could never have taken note of the fall of his apple" (**9**, p. 30).

The process of defining in science has recently come in for some serious overhauling. First to take up the attack, Bridgman may now see his labors continued by a number of scientists in different fields, particularly by Stevens (**10**, **11**, **12**) in psychology (*cf.* **6**). However, the logician has not remained silent; indeed his voice has sounded clearly and persuasively (**5**, **8**). Yet it is difficult to understand the scientist's preoccupation with definitions, unless it be that science is still young. Did not the Copernican and Cartesian revolutions put an end to the Aristotelian quest after essences? How else could one interpret the admonition of Leonardo: "The definition of the whatness—the quiddity— of elements is not in the power of man, but many of their effects are known."

Those who seek to put a question to nature need not be hampered by any lack of rigorously defined concepts, provided they are armed with a clear-cut question. "Die Natur eine zahlenmässig präzise Antwort erteilt, wenn man ihr eine präzise Frage stellt" (**2**, p. 193). It is in fact quite demonstrable (i) that definition in science is the end-product of research, not a prerequisite to it. Otherwise, it could not be shown (ii) that any definition is either true or false (**5**); nor (iii) that it is relative to a specific contextual system in which the definition is constructed. An elucidation of these three propositions and their applicability to psychology are the aims of this paper.

Definitions Derive From Research

What is perhaps more a contribution in the direction of emphasis rather than a new *genre* of thinking appeared in Bridgman's *The Logic of Modern Physics* a decade ago. In this work, alternately heralded and attacked, Bridgman formulated operational definitions of space, time, and length, and affirmed that a concept is nothing more than a set of operations, or even synonomous with the corresponding set of operations (1). Where no operation is possible, Bridgman insisted, there is no meaning. This impulse initiated in physics has been carried forward in psychology most vigorously by Stevens, who takes as his thesis Nagel's argument that "no statement or proposal has any meaning apart from the methods which are or may be employed to establish or execute them" (7). Stevens has formulated operational definitions of equality, meaning, existence, experience, sensation, attribute, and—quite inevitably—of definition (10). Empirical propositions, according to Stevens, are those testable by concrete operations. Like him, a number of other psychologists have offered operational definitions of temporal perception, learning, drives, etc.

Just what has been accomplished by these investigators infected with the virus of operationism (a term, incidentally, avoided by Bridgman)? Simply this: They have re-affirmed the fact that for science "the explorable nature of anything consists in its relations to other things . . . [since] science views facts not as isolated or separate events, but as connected in essence" (4, p. 107). For an operationist to argue that any proposition is a definition which records our decision as to how we shall use a particular term, is to *re-word*, not to revise, the methodology which science has consistently embodied in scientific inquiry. That is to say, these propositions have long since been established in scientific thought: a) that any definition, x, is an assertion whose truth or falsity can be determined; b) that any definition, y, which involves an assertion but does not specify how the truth or falsity of y should be ascertained, is not a meaningful definition.

Definitions do not represent what scientists say they do,

but are the crystallization of their actual achievements. That they must be constructed in terms of the procedures used to establish them is only incidental to the fact that they must stand the test of truth or falsity. Thus the spirit of competent scientists has ever been to "dismiss as thoroughly incompetent any proposal to assign meanings to such propositions as that light is corpuscular or that space is curved which neglects the complicated symbolic transformations needed before such statements can be given existential reference or which fails to note the technological apparatus required in order to implement them with a physical sense" (8, p. 578). Hence, the introduction of definitions into science bears a direct relation to the increase of rigor in science.

Operational definitions, then, point in the direction of scientific growth, since any concept so defined yields an analysis of its meaning which may be shown to be correct or not. If the analysis of the meaning is correct, the definition is true; if not, it is false. Perhaps these relations of truth and falsity in definitions lend additional perspective to the quarrel between the material monists in psychology and the introspectionists, since the former supposed that a science of human conduct could be built up without reference to controlled introspection. For obviously the measurable physical expressions of the organism with which experimental psychology deals, as compared to the data of introspection, permit of a greater number of operations. At any rate, when psychology was divorced from biology early in its career, it was in danger of becoming a barren and fruitless study of mental states, their analysis and integration. In a large measure this was inevitable when problems were too hastily defined with an air of finality. Those who followed McDougall's lead in the armchair construction of lists of 'instincts' or elementary impulses out of which society is presumably erected, in each case employed the same method. On the basis of insufficient research, a principle was formulated, and definitions were postulated; whereupon, a search was instigated to amass further facts to meet all rivals. Adherence to the proper rôle of definition, in psychology as in all sciences,

might have obviated this *cul-de-sac*, and August Comte would then not have been as convinced as he was of the metaphysical nature of psychology.

Operational definitions have also made a contribution in their emphasis upon experimental techniques, as has already been pointed out (**6**). It is indeed quite understandable that those concepts should tempt the operational viewpoint which have been subjected to a prolonged and varied treatment in the laboratory. By the same token, we should expect operational definitions of *personality*, *attitude*, and the like to be long in the making. This ought not prevent their usage nor defer their investigation (nor has it!); the fact that Euclid used a great many terms for which no rigorous definitions were offered in no way stultified his geometry nor rendered it less systematic.

Definitions are Relative

It is entirely consistent with the relegation of definition to the culmination of research that it should have a frame of reference. This frame of reference constitutes its proper context or system. Hence two definitions, a and a', incompatible in the same system, may both be true if they are each construed in the proper context to which each is in turn related. Or, two definitions, b and b', incompatible in two different systems, may both be true in the same system. In each case, the pairs of systems, though different, are in themselves compatible.

Before proceeding with a number of illustrations, it might be well to clarify the arguments above. What has been said is that any truth need not be limited to any particular language, nor is there any reason to suppose that truth can be expressed in only one language.[1] Scientists who seemingly make antagonistic assertions do not necessarily disagree, for they may be talking different languages. Truth is dependent upon facts for its perspective and therefore an assertion is true or false only within a specific context. Poincaré may have unduly exaggerated when he said that "all the scientist

[1] As used here, language is construed in its generic sense.

creates in a fact is the language in which he enunciates it," yet there is much to say of this language. If we avow that definitions are useful for certain purposes, it is incumbent upon the language in which the definition be embodied to demonstrate this purpose. The method of systematic relativism (**3**) referred to above is applicable to all rational systems where the language employed must be addressed to one system, since there are many systems, and in the medley of things we may become distracted from the obvious.

Thus, the principle of the relativity of definitions becomes quite instructive when applied to psychology. In defining learning, for example, it makes a difference as to whether or not the rubrics of the definition are derived from the system of physiology or from gross observable behavior, a system not comparable directly with neural physiology. While these systems seem widely divergent, they may nevertheless overlap in terms of the measurable data each seeks to amass, and in terms of the experimental techniques each may adopt.

Again, one may choose to define fear and anger as emotions, denying this definition to hunger and thirst; yet physiology has demonstrated a unique organic state in each of these. Obviously, they are all compatible in a system stressing these organic states. If we continue to make any distinctions between them, it is because we have chosen to relegate hunger and thirst (or fatigue, drowsiness, etc.) to one context, and fear and anger to another. No *real* distinction need necessarily exist; and the method of attack employed in the laboratory may not recognize any distinction at all.

Still another example may be taken from social psychology: that behavior of an individual may be defined as atypical which is not shared by the majority of the group as a whole. Departing from this concept, *i.e.*, of shared behavior, one may choose to define such behavior as a statistical deviation where the limits may or may not be clearly marked. It is apparent, however, that both definitions quite adequately explain why it may be that an individual's behavior frowned upon by one cultural group could elicit only the approbation

of another. This explanation would not be possible, however, if abnormal behavior were defined in terms of the individual's own inherent failings, unless it be assumed that the failings operative in one group would not be so in another group, of different culture.

What holds for definitions in psychology is equally applicable to explanations of psychological phenomena. The latter have not occasioned as much controversy as have definitions in psychology, inasmuch as they have usually assumed an air of inference, and as such are displaceable by other inferences. Finally, it should be mentioned that the very classifications in psychology demonstrate the principle of relativity, *e.g.*, child psychology (behavior relative to a genetic system), comparative psychology (behavior related to a system of infra-human organisms), or abnormal psychology (behavior associated with a system of atypical activity).

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CONDITIONED RESPONSE DATA AND THE HOLISTIC POINT OF VIEW[1]

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Introduction.—In view of the fact that the holistic principle as interpreted by Gestalt Psychology has been advanced as a general principle of organismic behavior, it is strange that the proponents of this system have not attempted to apply it, one of their favorite tenets, to the data of conditioned response experiments (which, after all, are established laboratory facts, not justifiably ignored by any fundamental theory). It is the writer's belief that such an application is possible, and that the holistic principle and the behavior which occurs in the conditioned response situation are not at all incompatible. Nevertheless, writers with a Gestalt slant have very largely avoided mention of conditioning data. A few examples out of many will suffice to show this. Koffka, in his *Principles of Gestalt Psychology* (**17**), devotes only a very few lines to conditioning, and Goldstein, in *The Organism* (**12**), includes only five pages on the topic. Such allusions to conditioning as are present almost invariably consist in the knocking down of an old man of straw, *i.e.*, the idea that conditioned responses (or conditioned reflexes as these writers prefer to call them) are inflexible units which, chained together, comprise all behavior. No one can deny that some years ago the conditioned response was sometimes used to explain behavior in terms of the chaining together of units, and, since such a point of view is the direct antithesis of holistic postulates, it is no wonder that Gestalt authorities disagreed with it. The fact remains, however, that, for ten years and more, there have been appearing a number of experimental and

[1] The writer wishes to express appreciation for criticisms and suggestions given by Dr. Lawrence I. O'Kelly, of the University of Colorado, and by Dr. William C. Biel, of Ohio State University.

theoretical articles by psychologists working in conditioning which are counter to any such notion of the nature of the conditioned response, and which demand rather a broader, more flexible interpretation of the phenomenon. It is the purpose of this article to consider some of these studies, and to demonstrate that the conditioned response concept based on experimental data, far from being in opposition, may actually supplement and support the holistic point of view, and may in turn be enriched by it.

Since misconceptions regarding the definitions of terms frequently prove to be the villains of the piece, it would perhaps be well to consider definitions before proceeding further.

The following quotation from Wertheimer (7) seems to state, as clearly as is necessary for our purposes, a definition of the holistic doctrine. He writes:

> There are wholes, the behavior of which is not determined by that of their individual elements, but where the part processes are themselves determined by the intrinsic nature of the whole. It is the hope of Gestalt theory to determine the nature of such wholes.

In other words, the response of any segment of the body is determined not only by local conditions within or immediately surrounding that segment, but by conditions of the entire organism.

It is also necessary to clarify what is meant by a conditioned response. It is obvious that the definition of the conditioned response must arise from the data which are obtained in typical conditioning experiments. In the typical conditioning situation the organism is repeatedly presented with two stimuli in definite time relations to each other. Both of these stimuli produce responses, though the response to one of them is usually, though not necessarily, more definite than the response to the other. If, after a varying number of presentations of these paired stimuli, one of them now produces a response which is *somewhat like* the response that was produced by the other stimulus, then it is said that

the organism has become conditioned. With a slightly different emphasis, Hilgard has said (**14**):

> The sources of conditioned response data will be limited to laboratory experiments in which there occurs an alteration in response tendencies in respect to a (conditioned) stimulus by virtue of its repeated presentation in a controlled relationship with another (unconditioned) stimulus which with relative regularity evokes a response from the beginning of the experiment. The 'alteration in response tendencies' to be described as conditioning refers either to the appearance of a novel response or to the modification of a response originally evoked by the conditioned stimulus. Whether the unconditioned response is to be described as a reflex is unimportant so long as the response consistently occurs.

It is well to emphasize the fact that the conditioned response is only 'somewhat like' the response that was produced by the unconditioned stimulus, and that neither the writer's definition nor the one advanced by Hilgard says anything about a specific stimulus producing a specific response, since experimentally that is not the case. This, however, is one of the most misrepresented aspects of the conditioning situation, and its misrepresentation has unfortunately contributed much to the disagreement between the holistic psychologist and the conditioning experimenter. Writers from a Gestalt point of view have frequently inferred the very thing against which we have just cautioned (**1**, **12**, **17**, **24**), namely, that in a conditioning situation a specific stimulus must produce a specific response. Any postulation of an invariable particular response to a particular stimulus is, of course, not in accord with holistic doctrine. Adopting a holistic point of view, one would predict rather that responses would be variably effected by varying conditions in the organism and the situation. One might expect that different, individual effects upon the final response might be produced by the attitude of the subject, for example, or by his particular motivational state, or his general physiological condition, or, perhaps, by

his perceptual organization of the field.[2] In the light of these possible expectations from the holistic point of view, it will be worth while to consider some of the actual data from conditioning experiments which touch upon the same problems.

Attitudes in Relation to the Conditioned Response.—Of the many problems which have constantly bedeviled experimenters in conditioning, the control of the subject's attitude during the conditioning situation has been one of the foremost. Examples of this are legion (**3**, **4**, **13**, **15**, **20**, **21**, **23**). A study which has specifically attacked this problem is one by Hilgard and Humphreys (**15**). After developing a simple conditioned eyelid response to light with an air puff to the cornea as the unconditioned stimulus, these experimenters had their subjects proceed to the development of conditioned discrimination reactions. Before attempting the discrimination, the subjects were divided into four groups. To the control group, group B, no verbal suggestion was made, other than that contained in a description of the general procedure. To each of the other three groups, a different attitude was suggested: Group *C* was told to wink to the positive stimulus (a light which was always reinforced) and not to the negative (another light which was never reinforced), while the opposite directions were given to group *D*; the last group, *E*, was asked to inhibit responses to both the positive and negative stimuli. Here we have a situation which makes it possible to determine whether or not the total condition of the organism may effect the conditioned response. If the conditioned response is a simple reaction to a simple stimulus quite uninfluenced by the total condition of the organism, then all of our groups would give similar results; the holistic principle, however, would predict variations in the percentage of response from situation to situation.

Such predicted results actually occurred in the experiment. In general we found that a different attitude produced a distinct quantitative difference in response. Table I, compiled from Hilgard and Humphrey's results, demonstrates

[2] The categories and classifications here used are necessarily more or less arbitrary ones, and it is not meant, of course, that they are mutually exclusive.

this fact. The left hand column represents the four different groups. The headings, Positive and Negative, designate whether the response was made to the positive or to the negative stimulus. The subheadings beneath these main headings indicate the attitudes of the various groups, *i.e.*, whether directed towards responding, directed against responding, or neutral (control group), along with the corresponding frequency of response in percent. It is apparent from the table that those groups which were asked to adopt a negative attitude to either one of the stimuli showed a lower frequency of response to the stimuli toward which that set was directed than the groups whose attitude was positive. A specific comparison between groups *D* and *E* more strikingly

TABLE I

A TABLE COMPILED FROM THE DATA PRESENTED BY HILGARD AND HUMPHREYS IN THEIR ARTICLE, THE EFFECT OF SUPPORTING AND ANTAGONISTIC VOLUNTARY INSTRUCTION ON CONDITIONED DISCRIMINATION

Group	Positive		Negative	
	Attitude	Per cent Frequency	Attitude	Per cent Frequency
B	0	74.2	0	25.9
C	+	90.1	−	13.0
D	−	71.1	+	94.0
E	−	55.3	−	19.4

indicates the complicating effect of the whole situation. From these facts it is clear that the magnitude of the conditioned response is not determined solely by the pairing of conditioned and unconditioned stimulus, but by the total state of the organism.

The fact that attitudes are important not only in the continued appearance of the already established conditioned response, but also in the actual establishment and building up of conditioning is demonstrated by an experiment of Grant's (**13**). Subjects who were instructed to maintain an active attitude toward the appearance of the conditioned stimulus were found to develop conditioned responses much more rapidly than those who were requested to remain

passive. An experiment just reported by Miller (**20**) specifically illustrates the importance of verbal attitudes in the establishment of the conditioned response.

It is of incidental interest that these studies, while being made more meaningful by the organism-as-a-whole concept, also serve to make that concept itself more meaningful. They give, in fact, rather precise quantitative information concerning the manner in which certain total conditions of the organism may operate. Indeed, it appears from these studies that the conditioned response situation might serve as a valuable tool with which to investigate the effect of changes in the condition of the (whole) organism. To the writer it seems that the organism-as-a-whole concept has too frequently been used in vague and meaningless ways; but here may be seen almost unlimited possibilities for giving this concept real experimental meanings. There is an indefinite number of total conditions of the organism which may be established, and the effect of any one or all of these conditions on the large number of highly different part processes or particular aspects of the whole which have been conditioned (GSR, pupillary contraction, eyelid response, defense movements, simple reflexes, vasomotor responses, etc., **16**) would surely tell us much more than we now know about the behavior of organisms.

Interactions between Conditioned Responses.—Attitudes, however, are not the only conditions of the organism which have their quantitative effect on these part processes; other conditioned responses themselves do so, as is clearly demonstrated by an experiment of Rodnick's (**22**). This experimenter developed independently in his subjects a delayed galvanic skin response of about ten seconds with a light as the conditioned stimulus, and also a short latency (about 500 ms.) eyelid conditioned response with a vibratory stimulus to the skin as its conditioned stimulus. After both of these responses had been well established, the light was presented, to which the galvanic deflection usually occurred about ten seconds later. During this delay, the organism, according to Pavlov, is making a definite inhibiting response. In other

words, its total condition is different from its total condition immediately before the light stimulus occurred and after the response was made. Now, during this interval, Rodnick presented his other conditioned stimulus, the tactual vibration. He found that, although the subject did make the usual conditioned response to this stimulus, *i.e.*, a closure of the eyelid, the data indicate that the condition of inhibition imposed by the first stimulus produced a quantitative decrement in the intermediate response. The amplitude of the eyelid conditioned response was reduced by about 19 per cent, while its latency was similarly increased by about 17 per cent.

It is again apparent that a part process is influenced by the total condition of the organism in which it is imbedded. However, in Rodnick's study, the organismic pattern which modified the part process was itself largely determined by another *conditioned response*. The fact that this conditioned response had its genesis under the eyes of the experimenter and could be strengthened or weakened by the experimenter makes the results of unusual scientific interest, and opens up wide possibilities for further investigation. The strength and nature of not only the part process but also of what, in this experiment, helped to determine the total conditions, are both under the control of whatever experimental variable the researcher may wish to use. Once more there are apparent in the conditioning technique extensive opportunities for discovering something more about wholes and their action upon subservient part processes.

Specific Organic States in Relation to the Conditioned Response.—From the old stronghold of salivary conditioning there is available a large mass of further evidence to support the view that the response to the conditioned stimulus is determined by factors other than such purely mechanical ones as the frequency with which the conditioned and unconditioned stimuli have been paired. Lashley (**18**), Finch (**8**), Zener (**28**),[3] and Zener and McCurdy (**29**) have all shown

[3] On the basis of his experiments, Dr. Zener has expressed a point of view which we believe to be similar to that set forth in this article.

that the strength of the hunger drive within the organism will be effective in determining the magnitude of the conditioned response. In Zener and McCurdy's experiment, for example, their experimental animals, dogs, were strongly conditioned and were brought into the experimental room on the test day with hunger drives of from 21 to 24 hours. Quantitative measurements of salivation to the conditioned stimulus were taken, and, following this, the animals were satiated with biscuit. After the dogs ceased to eat the food, the conditioned stimulus was again presented, the consequent salivation, of course, being measured. In this case, however, there was a marked difference in quantitative measurements. The amount of salivation in this satiated condition had dropped to something like 15 or 25 per cent of what it was previous to any satiation. A similar observation was made by Finch (**8**), who, in addition, demonstrated that the relationship between the period of deprivation and the strength of the conditioned response was a parabolic function. In these studies, we have the same stimulus, the same external situation, and (by name at least) the same dog, but definitely different results. Again it is obvious that the total condition of the organism has effected the conditioned response.

Not only do we find that both great and small quantitative changes may be produced in the conditioned response by initiating changes in the total organism, but also experiments have shown that presence or absence of the conditioned response can be determined by the nature of the concomitant organic states.

In a study investigating the possibility of conditioning in the newborn infant, Wenger (**25**) was confronted with the fact—a not uncommon one in the field of conditioning—that at one moment the response would be present and at the next it would be absent, only to reappear an instant later. Continuing his observations, it became apparent to him that the presence or absence of the response was correlated with the condition of the baby at the time of stimulation. Now, the neonate fluctuates with great rapidity through various stages of activity. At one moment it is difficult to observe any

response other than breathing, while in the next, the infant appears like a vocalizing assortment of arms and legs jerking about in all directions. Between these two poles, a continuum of activity exists. Wenger noted that when the infant's behavior approached either of these extremes, the conditioned response in general tended to disappear, but, when the child lay fairly quiet, with only occasional movements, the response would reappear.

A highly dramatic experiment which is particularly pertinent to our approach is one by Girden and Culler (11) on the effect of curare on the conditioned response. Using dogs as their experimental specimens, these workers performed an operation which exposed the semi-tendinosus muscles of both legs. The muscle of the right leg was then conditioned to respond to a bell, shock having been the unconditioned stimulus. At this point the animal was curarized, and the conditioned response was found to be completely absent, despite the fact that the muscle would respond with a slight twitch to the application of shock to the toe pads. Using this slight twitch as the unconditioned response, a conditioned response to the same old conditioned stimulus was established in the left leg. When the animal recovered from the curarization some three or more hours later, the right muscle conditioned response returned, but the left muscle response was absent. Recurarization brought about the reversal of this, with the right response absent and the left response present. Changes in the organism produced by the drug caused radical changes in an already existent conditioned response, and, in turn, recovery from the drug brought about the disappearance of a response which had been established in the drug situation, and, with it, the reappearance of the first response.[4]

Both this study and the one quoted from Wenger demonstrate the great significance of the total organic state of the

[4] An explanation of these results at a physiological level has been advanced by Culler, Coakley, Shurrager, and Ades in a later study. It is the feeling of this author that such an explanation can be accepted without doing violence to an equally applicable holistic interpretation at another level (5).

individual in any appearance of conditioned response phenomena. Possibly also pertinent here are some of the studies which have been done on the establishment of neuroses in the lower animals by means of a conditioned response technique (6). With the disturbance of the total organism which indicated the nervous breakdown, there was likewise a disruption of the existent conditioned responses. They were not isolated units independent in their existence and expression.

Perceptual Factors in Relation to the Conditioned Response. —So far we have spoken only of the quantitative effect of changes in the organism-as-a-whole on the conditioned response. It is also true that the quality of the response may be effected, and that the muscular components of the response are determined not solely by the nature of the response to the unconditioned stimulus, but also by the total condition of the organism at the moment that the conditioned stimulus impinges on its receptors. For example, the nature of the original response to the unconditioned stimulus does not aid us in predicting the behavior of one of Bechterev's dogs (2). Bechterev, after having conditioned a withdrawal movement of one leg to shock, tied down that member and presented the conditioned stimulus. The animal, instead of vainly struggling to lift the restrained limb, as one might predict, promptly responded by making a withdrawal movement of the other foreleg.

Such contralateral transference of the conditioned response has been subjected to rather complete examination by Gibson, Jack, and Raffel (10). College student subjects were conditioned to withdraw the right hand from an electric grid at the sound of a buzzer. After the response had become established, the left hand was placed on the grid and the conditioned stimulus was sounded. Now, instead of obtaining the results which might have been predicted if the conditioned response consisted of a definite specific response to a definite specific stimulus, namely, a useless jerk of the right hand, it was found that eight of the 13 subjects responded by removing the left hand from the grid. The same general

problem was approached by Gibson and Hudson (**9**) in a later work in which they used the knee jerk as their unconditioned response.

Not only will one member of the body act as a substitute for another as a response mechanism, but in some cases we find that the conditioned member will react in a manner that is, in terms of the qualitative aspect of the response, the opposite of the response which was conditioned in the original training series. Several studies by the writer demonstrate this fact and offer additional evidence against any strict mechanical interpretation of the conditioned response in terms of muscular activity. In one experiment (**26**) 18 college students were conditioned to make an extensor movement of the middle finger when a buzzer was sounded, shock being the unconditioned stimulus. After each subject had become conditioned to a certain minimal strength, his hand was turned over so that a flexor movement of the same finger would be necessary if this digit were to be removed from the electrode. The buzzer alone was sounded, and ten of the 18 subjects immediately responded to the buzzer by appropriate flexor movements, rather than by the original extensor movement which had been conditioned. In a later study (**27**), transference of conditioned discrimination from one muscle group to its antagonist was also demonstrated in the same fashion. Liddell, James, and Anderson (**19**), working with sheep, have likewise demonstrated that the postural adjustment of the moment may determine the form of the conditioned response.

In all of the six studies just cited a body of experimental data consistently points toward the conclusion that perceptual factors within the organism must be taken into consideration when we attempt to predict the muscular components of a reaction. The conditioned response is not an isolated muscular unit separated from the rest of the organism, but it is a functionally significant reaction which is subject to modification as a result of the organism's perceptual organization of the field. Again it is found that the conditioned

response is not a part process alone, but an aspect of the organism-as-a-whole.

In the light of the facts that such total processes as we have mentioned—attitudes, motivational and drug conditions, other conditioned responses, the degree of integration of the total organism, and perceptual factors—all have an undeniable influence on the quantitative and qualitative nature of the conditioned response, it seems impossible to consider the conditioned response and the organismic view-point as discordant. Not only is there in existence a large mass of conditioning data (by no means all of which has been cited in this article) which is compatible with and which enriches the holistic doctrine, but, in addition, the conditioning situation seems to offer wide possibilities for further research on the nature of wholes and their influence on part processes. Since this is the case, it appears foolhardy for the psychologist with Gestalt leanings to continue to ignore this lively field; instead, he should begin to make efforts to fit it into as many aspects of his system as possible.

Just as it is unfortunate that the writer with the holistic point of view has frequently disregarded conditioned response data, so it is also grievous that experimenters in conditioning have neglected the opportunities which existed to see and understand their facts in relation to a prevalent system. In the holistic doctrine we have a flexible and general concept which may do yeoman service for many of the current problems of conditioning. Indeed, we may find in it a single unifying principle which will make it unnecessary to force the phenomena of conditioned response into rigid, possibly artificial categories such as types and levels of conditioning. It is not inconceivable that benefits might be gained both by the proponents of a holistic doctrine and by the experimenters in the field of conditioning, if each considered carefully the contributions of the other.

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SUPPORT FOR THE EXPLORING TONE METHOD OF MEASURING AURAL HARMONICS

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Contemporary auditory theorists quite generally agree that subjective harmonics and combination tones have physical representation in terms of actual vibrations in the cochlea. These vibrations, currently called aural harmonics, are held to be products of distortion in the ear. The accepted viewpoint, for which there is much confirmatory evidence, is that distortion occurs whenever sound pressures of other than small amplitudes are impressed on the ear, with the amount of distortion increasing with pressure amplitude. It is not possible, at present, to say with confidence whether the distortion is to be attributed primarily to nonlinear transmission properties of the middle ear or to nonlinear properties of inner ear mechanisms; but the 'fact' of distortion is taken for granted.

Aural harmonics, if they are generated at all, naturally have frequency, magnitude, and phase, and they probably are measurable. How measure them? Their frequencies, in the case of steady-state sound stimuli at least, can be inferred from theory (**2**). Is there any reasonably reliable way of determining their magnitudes and phases? Nothing specific about magnitude or phase can be inferred from what is now known about either the structure or functioning of the ear; and introspective analysis is admittedly of slight value. Of the possible methods of measurement, the *exploring tone method* is the only one which has been regarded favorably for use with normal human ears [1]; and it has been employed

[1] An electrical method involving the analysis of cochlear potentials has been developed by Newman, Stevens, and Davis (**6**) and by Wever and Bray (**9**) to measure aural harmonics in the ears of guinea pigs and cats. The method, especially as used by Wever and Bray, has served to reveal valuable information, but its application to investigations of normal human ears is apparently precluded.

with what seemed to be reasonable success. Recently, however, the assumptions underlying the method have been subjected to criticism by Trimmer and Firestone (7). The purpose of this paper is to evaluate Trimmer and Firestone's criticisms, especially in the light of experimental data which have been secured in the Iowa laboratory.

Two different procedures are possible in introducing the exploring (or 'probe') tone. To make the difference between the procedures clear, a specific example will be used. Let P_o represent a simple pressure wave of frequency f and amplitude A_o. ($P_o = A_o \sin 2\pi ft$.) The stimulus P_o gives rise to a cochlear displacement X_o. Presumably, if A_o is not too small, aural harmonics are generated whose frequencies are $2f$, $3f$, $4f$, etc. In other words, X_o is not simple but complex in form, and can be represented by a function of the Fourier type. It is desired, say, to measure the aural component whose frequency is $2f$. For this purpose, an exploring tone P_e is introduced. The oldest procedure, first employed by Wegel and Lane (8), provides for introducing a pure tone having a frequency of $2f \pm d$, with the increment d preferably about 3 cycles. ($P_e = A_e \sin 2\pi[2f \pm d]t$.) As a result, beats occurring d times per second are heard. The beats are said to be due to interference between P_e and the aural harmonic being probed. It has been established that when two sinusoidal waves having closely adjacent frequencies are combined in air and presented to an observer, 'best' beats (that is, most prominent beats) occur when the amplitudes of the two waves are identical. Consequently, the value of A_e which yields 'best' beats is assumed to be a measure of the magnitude of the aural harmonic under consideration. This procedure is sometimes called the *method of beats*.

The other exploring tone procedure, described originally by Chapin and Firestone (1), provides for introducing a pure tone with a frequency identical to that of the aural harmonic being studied. This procedure has been called the *steady tone method*. In the case of the illustration here used, P_e would have a frequency of $2f$. ($P_e = A_e \sin [4\pi ft + \theta]$, where θ is the phase angle.) Both the phase angle and the

amplitude of P_e are adjustable. A change in phase angle through a full 360° gives rise to extremes of change in the amount and kind of interference between P_e and the involved aural harmonic. The observer listens for changes in loudness. Loudness is a maximum when there is maximal constructive interference, and a minimum when there is maximal destructive interference. The phase settings for maximal and minimal loudness are 180° apart.

One of the difficulties of the steady tone method, as outlined thus far, is that noticeable changes in loudness do not appear, when phase is altered, unless the amplitude of P_e is properly adjusted. This generally requires a simultaneous manipulation of phase and amplitude. Another difficulty is that while the phase angle of the aural harmonic can be computed approximately, computations of magnitude are not feasible. To overcome these difficulties, Lewis and Larsen (4) modified the procedure so as to measure, for various selected phase angles of P_e, amplitude thresholds of P_e, at which levels the observer could barely detect a change in the loudness of the aural harmonic involved. As will be shown in more detail later, information thus secured can be used to compute both phase angles and magnitudes.

It should be apparent that the use of either procedure in introducing the exploring tone is not limited to circumstances in which P_o is a simple pressure wave. P_o may be either simple or complex in form. Whatever its form, there will arise a certain cochlear displacement X_o. The important thing is that X_o will contain certain elements which have no physical representation in P_o. Any one of these elements may be investigated by a suitably chosen exploring tone P_e. The only restriction is that P_e be sinusoidal.

In typical experiments in which the exploring tone method is used, P_o and P_e are transmitted simultaneously, over the same distorting aural channel. Let P represent the pressure wave when P_o and P_e are combined, and X the resulting cochlear displacement. Then, $P = P_o + P_e$, and $X = X_o + X_e + X_{oe}$, where X_o is the cochlear displacement due to P_o alone, X_e the displacement due to P_e alone, and X_{oe} the

displacement due to the simultaneous transmission of P_o and P_e. Two important assumptions are ordinarily made. One is that X_{oe} is negligible. The other is that any change which occurs when P_e is varied in either amplitude or phase consists very largely, if not entirely, of a change in the component of X which has the same frequency as P_e. Both of these assumptions have been questioned by Trimmer and Firestone (7). They state, in effect, first, that X_{oe} is not negligible, and second, that a change in P_e (through a full 360°, for example), instead of introducing an outstanding variation in the component heard at the pitch of P_e, causes a 'complex cycle of quality and loudness changes,' of which the change in the prominence of the fundamental (the frequency of P_o) is most noticeable. Obviously, if this statement is accepted as descriptive of the essential facts, the exploring tone method is not valid for measuring the phases and magnitudes of aural harmonics. A careful scrutiny is needed.

It is not at all surprising that the observers used by Trimmer and Firestone, and those used earlier by Chapin and Firestone, perceived 'a complex cycle of quality and loudness changes' when the phase angle of P_e was varied through 360°. In all of their experiments, Trimmer and Firestone used, for P_o, a simple frequency of 100 cycles with an intensity level of 104 db, a sound which is usually heard as unpleasantly loud. A typical observer cannot 'hear out' the various aural harmonics of such a sound. Rather, a massive complex of loudness is heard and is usually referred to the fundamental component. And yet, when Trimmer and Firestone introduced a second simple frequency of 200 cycles and varied its phase, they apparently expected the chief experiential change to occur in the aural harmonic of 200 cycles even though this component was not specifically audible to begin with. There is no reason for expecting the average person (or even a person carefully trained in auditory analysis) to hear every partial in a tone for which there is a cochlear component. In fact, Lewis and Lichte (5) have recently shown that trained observers frequently cannot

describe the difference between two complex tones (in terms, say, of the strength or weakness of specific partials) even though they readily hear the two tones as different in timbre. This finding does not mean that each partial does not contribute its part to the complex tonal experience. It no doubt does mean that a partial can, in some instances at least, be reduced or increased in magnitude without a concomitant experience of change in that partial. Trimmer and Firestone apparently supposed that because their observers, while altering the phase of P_o, did not hear important changes in the component having the frequency of P_e, the changes they did hear were not due to alterations in that component alone but to important alterations in other components also. The evidence they offer in support of this supposition is the fact that their observers heard 'a complex cycle of quality and loudness changes.' But such changes conceivably could be due primarily to a modification in a single aural harmonic. It must be admitted, of course, that when both P_o and P_e are introduced at high intensity levels, not only X_o but also X_e is complex in form. Furthermore, a change in the phase angle of P_e alters the phase of all components of X_e (and of X_{oe} also, for that matter). Even so, it seems reasonable to believe that the changes which Trimmer and Firestone's observers perceived may have been due mainly to alterations in the strength of the aural harmonic whose frequency was the same as that of P_e. However, it is not necessary to accept this conclusion in order to justify the retention of the exploring tone method for restricted use. It could be granted that the method is not entirely satisfactory for use when P_o has an intensity level such as that used by Firestone and his colleagues, and still be said that it has validity under some circumstances.

The principal features of the exploring tone method as employed in the Iowa laboratory to measure the phase and magnitude of aural harmonics will now be presented. In the production of both P_o and P_e, use is made of an electrostatic generator, which has been described elsewhere (**3**). With this instrument, it is possible to produce sounds constituted

of any or all of 16 natural harmonics, the frequencies, intensities, and phases of which are subject to control. Each of the individual harmonics is reasonably free from impurities, the strongest stray component being down at least 40 db.

In the hearing tests, a modification of the constant process is ordinarily used. A stimulus tone, on which judgment is made, has a duration of about 4 seconds. It has two parts: a *standard portion* and a *variable portion*, each about 2 seconds in duration. The standard portion is a specified sound, either simple or complex, of predetermined structure, while the variable portion is always the standard portion *plus* one additional harmonic whose intensity level and phase may be altered independently. In terms of symbols, the standard portion of the stimulus tone is P_o, and the variable portion is $P_o + P_e$. The generator is so constructed as to permit as many as five different intensity levels of P_e to be set in advance for an observational series; and 100 stimulus tones, in which the variable portion occurs at random (that is, sometimes before and sometimes after the standard portion), may be presented more or less automatically. The nature of the judgment an observer is asked to make will be indicated below, in connection with a specific example. The generator circuit is arranged to avoid audible transients both when a tone begins and ends and when the standard portion changes to the variable portion, or vice versa. A calibrated Western Electric 555 receiver is the actual sound source; so it is possible to express the magnitude of any component in a sound in intensity level units (db above 10^{-16} watt).

A description of a typical experimental series should help to clarify the basic procedure. Two simple frequencies, one of 520 and the other of 650 cycles per second, were combined for monaural presentation. They were each set at an intensity level of 70 db and were in phase (that is to say, they were sine functions without epoch angles). These two frequencies may be conveniently thought of as the 4th and 5th harmonics of a Fourier series with an assumed fundamental of 130 cycles. In combination, the two frequencies gave rise to an experience of a complex tone in which, among

other things, an *aural* harmonic with a pitch corresponding to 130 cycles was specifically audible. That this perceived harmonic had no effective counterpart in the stimulus tone was shown by the fact that the fundamental component of the stimulus had an intensity level of only 2.5 db. In other words, the 'stray' fundamental was 67.5 db below the level of the 4th and 5th components, and it was at least 20 db below its threshold of audibility. It could, therefore, be regarded as inconsequential. The particular aural harmonic under consideration may be called either a subjective fundamental or a first-order difference tone. It will be referred to, here, as a subjective fundamental.

The first step in determining the phase and magnitude of this subjective fundamental was to measure the *masked absolute threshold* of the objective fundamental as a function of the phase angle of the latter. The standard portion of the stimulus tone (P_o) was composed of the 4th and 5th harmonics, with intensity level and phase as indicated above. The variable portion ($P_o + P_e$) was composed of these same two harmonics in addition to some specified amount of the 1st harmonic at a given phase position. The 1st harmonic (the objective fundamental) was the exploring tone. An observer was asked to judge whether the fundamental component was louder in the first or the second half of the stimulus tone. It should be said, in view of the criticisms of Trimmer and Firestone, that *the observers heard the difference between the two portions of a stimulus tone as being in the magnitude of the fundamental component.*

On the basis of a large number of judgments, computation was made by linear interpolation of the intensity level of the 1st harmonic at which the fundamental component was heard, in 75 per cent of the trials, as louder in the variable portion than in the standard portion. This intensity level value, called in this instance the masked absolute threshold of the 1st harmonic, was found to vary with phase, as Fig. 1 indicates. The phase effect may be attributed to interference between the subjective and objective fundamentals (that is

to say, between the subjective fundamental and the exploring tone).

The data presented in Fig. 1 were obtained on the left ear of observer B. Masked absolute threshold, expressed in terms of intensity level, is plotted against phase angle in degrees. A smooth curve has been drawn through the computed points, each of which was based upon at least 300 judgments. The broken horizontal line shows the location

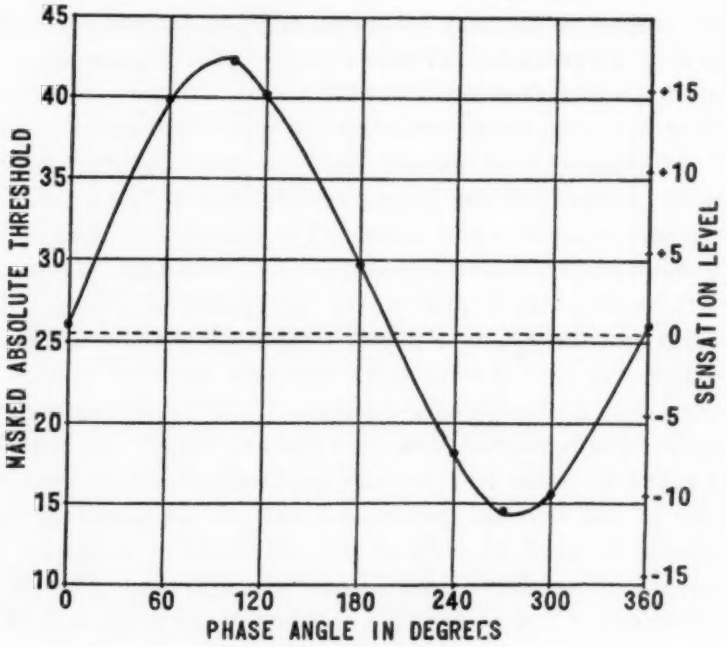

FIG. 1

of the *threshold of audibility* for the 1st harmonic, which was measured in the absence of masking. Sensation level (db ± threshold of audibility) is indicated by the ordinates at the right. It will be seen that, for a phase angle of 0°, the masked absolute threshold of the exploring tone was 26 db. Other phase settings yielded other threshold values. The curve reaches a maximum at about 95°, the threshold here being close to 42.5 db. It is at a minimum at approximately

275°, where the threshold is near 14 db—at least 11 db below the threshold of audibility. The difference between the highest and lowest threshold values is about 28 db. The pressure values which correspond to the extremes are .0267 and .0011 bar (dynes/cm.^{2}).

The change in masked absolute threshold with phase angle is attributed to interference between the aural harmonic and the exploring tone. Destructive interference, in this case, undoubtedly occurred maximally at about 95° and constructive interference maximally at about 275°. The apparent phase of the subjective fundamental was, therefore, close to 275°. The fact that the lowest masked absolute threshold value was so far below the threshold of audibility makes an explanation of the results in terms of interference particularly compelling. Additional evidence to support an explanation in terms of interference has been presented by Lewis and Larsen (4). With the proper adjustment of the phase and magnitude of an exploring tone, they were able to cancel out completely a subjective fundamental which had been clearly audible in the absence of the exploring tone. In other words, their two observers perceived a fundamental component in a two-harmonic sound which lacked an objective fundamental, and yet they heard no fundamental component at all when the same sound w s presented with an added objective fundamental of required phase and intensity level. And the intensity level of this added objective fundamental, in one instance, was about 45 db (close to 20 db above its threshold of audibility).

The conclusion is reached that the phase angle of an aural harmonic is given by that phase angle of the corresponding exploring tone at which the masked absolute threshold of the exploring tone is a minimum.

The computation of the magnitude of an aural harmonic rests on knowledge of both maximal and minimal masked absolute threshold values for the exploring tone. Within the margin of experimental error, maximal and minimal values are obtained for two phase angles of the exploring tone which

lie 180° apart. The phase angles themselves do not enter into determinations of magnitude; only the threshold values obtained at critical phase angles.

The method of computing magnitude, as already presented by Lewis and Larsen (4), is made clear by reference to Fig. 2. Consider condition [1]. Pertinent effects which arise during the two halves of a stimulus tone are represented, with those arising during the standard portion to the left and those during the variable portion to the right of the broken vertical line. Let S represent the magnitude of the subjective fundamental and be the equivalent of an unknown amount

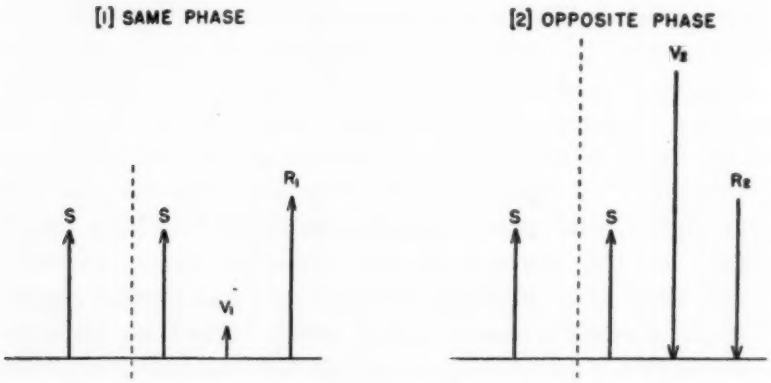

FIG. 2

of sound pressure, and let V_1 be a measurable amount (pressure) of the 1st harmonic (the exploring tone). As the arrows indicate, S and V_1 have the same phase. In the variable portion of the stimulus tone, S and V_1 combine to give rise to the resultant R_1. If V_1 is the masked absolute threshold pressure of the 1st harmonic (for the 'in-phase' position), R_1 will be heard as just perceptibly louder than S. Now,

$$R_1 = S + V_1. \tag{1}$$

In condition [2], V_2 is shown as opposite in phase to S. It is also opposite in phase to V_1. V_2 and S, together, produce R_2. When V_2 is the masked absolute threshold pressure,

R_2 will be heard as just perceptibly louder than S. It is clear that

$$R_2 = V_2 - S. \tag{2}$$

The value of S is desired. Obviously, V_1 and V_2 are the only quantities represented in the above equations which are experimentally measurable. However, for threshold conditions, R_1 and R_2 are each heard as just perceptibly louder than S and are probably equal in magnitude even though 180° out of phase. If they are assumed to be equal, with phase justifiably disregarded, then from equations (1) and (2), it follows that

$$S + V_1 = V_2 - S,$$

$$S = \frac{V_2 - V_1}{2}. \tag{3}$$

The application of equation (3) to the data summarized in Fig. 1 yields a value for S of .0128 bar. If the equation is valid, this pressure value indicates that the subjective fundamental, in the ear of the observer used, was essentially the equivalent of a cochlear disturbance that would be set up, in the absence of other stimulation, by a simple sound wave with a frequency of 130 cycles and a pressure of .0128 bar. The intensity level corresponding to the computed pressure value is 36.1 db.

Let it be assumed, tentatively at least, that the subjective fundamental, as generated in the left ear of observer B under the conditions of the experimental series, had a phase angle of 275° and a magnitude corresponding to an intensity level of 36.1 db. There is apparently no way of checking the correctness of the assigned phase angle—except possibly by inference provided the designated magnitude can be checked. With regard to verification of the computed magnitude, it can be pointed out that observer B heard the subjective fundamental clearly. He reported that it was relatively soft but nevertheless quickly distinguishable. He had had extensive experience in auditory observation, including frequent

use of the 2-A audiometer; and he estimated, before computations had been made, that the fundamental component sounded as if it were 8 or 10 db above its threshold of audibility. His threshold of audibility (left ear) for a frequency of 130 cycles was 25.5 db; so his estimate of the above-threshold level of the subjective fundamental coincided quite well with the computed magnitude.

Further substantiation for the proposed method of computing magnitudes can be given by reference to the data in the table. In this table, phase angles and magnitudes are

PHASE AND MAGNITUDE VALUES FOR SUBJECTIVE FUNDAMENTALS, AS MEASURED WITH AN EXPLORING TONE OF 130 CYCLES

Experimental Series	Stimulus Frequencies		Phase Angle		Magnitude in db*	
			Right Ear	Left Ear	Right Ear	Left Ear
Observer B:						
I	260	390	330	320	43.3	43.5
II	390	520	330	320	41.6	40.1
III	520	650	290	275	36.7	36.1
IV	650	780	230	230	28.7	23.0
V	780	910	200	60	23.9	15.4
Observer A:						
II	390	520	180	200	32.1	30.6
V	780	910	150	90	32.1	37.3

* db above 10^{-16} watt.

shown for 14 different subjective fundamentals: 10 for observer B (5 in each ear), and 4 for observer A (2 in each ear). Each subjective fundamental arose when the observer listened with the specified ear to two simple frequencies in combination. The two frequencies used in each series are listed under 'Stimulus Frequencies' in the second column of the table. They were in phase and were each at an intensity level of 70 db. The computed values given in the table reveal many things of general interest (for example, the marked differences between phase and magnitude values for the two observers in identical series; the similarity of the values for the two ears of observer B in series I, II, and III, and the two ears of observer A in series II; the large

discrepancy between the phase angles for the two ears of both observers in series V; etc.), but of particular importance for the purpose at hand are some of the computed magnitudes.

For series IV, with stimulus frequencies of 650 and 780 cycles, the magnitude of the subjective fundamental in observer B's right ear was computed to be 28.7 db; in his left ear, 23.0 db. When he listened to the stimulus with his right ear, he heard the fundamental component. It seemed very faint, he reported, but nevertheless distinguishable. But when he listened to the same stimulus with his left ear, he failed to detect a fundamental. This difference in his experience tends to substantiate the correctness of the two computed magnitudes. The threshold of audibility for his right ear for a frequency of 130 cycles was 25.8 db; for his left ear, 25.5 db. In his right ear, then, the subjective fundamental was, by computation, about 3 db *above* the threshold of audibility; in his left ear, it was 2.5 db *below.* The computed values are thus given support by B's statements on his experience. In series V, he never heard the fundamental component when listening with either ear; and he always heard it in series I, II, and III. It was particularly distinct for him in series I and II.

The reports of observer A are also pertinent. With left-ear listening in series II, he was usually in a quandary. Maybe the fundamental was audible; maybe not. He could not say for sure. When he listened with his right ear in either series II or series V, he was more positive in his reports. The fundamental seemed to be there, he said, but it was not easy to distinguish. He wished that it were just a little bit stronger so he would not have to strain himself quite so much to keep track of it. Only in series V, when he listened with his left ear, did he feel certain that the fundamental component was consistently audible; and even in this case, it was not very strong. He and observer B, on one occasion, came close to strongly-worded disagreement. They were listening to series II with their left ears, taking turns in making groups of 100 judgments. Observer B reported that the fundamental component was quite strong. A did not agree: he said that

it was very weak, if present at all; and he finally informed B that he was 'hearing things.' B replied that A did not know what to listen for. Both of them were right, and both wrong—if computed magnitudes are accepted as approximately correct. A's right-ear threshold of audibility was 26.0 db; his left-ear threshold, 28.8 db. For him, in series II, the subjective fundamental, in his left ear, was at most only about 2 db above threshold. For B, in the same series, the fundamental was at least 15 db above threshold.

So far as present information goes, the computed magnitudes given in the table are in the right direction and are probably reasonably correct. The procedure used to verify them is much more qualitative than might be desired, and there is some possibility that they could be checked more exactly. Perhaps, in those circumstances when a subjective fundamental or some other aural harmonic was specifically audible, an observer could be instructed to adjust a simple sound of the same pitch until it was judged to be equal in loudness to the aural component. Then, if adjusted values were found to agree reasonably well with computed values such as those in Table I, the exploring tone method as used in the Iowa Laboratory would be given more complete justification. However, even without this additional support, the method, if used with caution, seems to be defensible. The criticisms of it which have been made by Trimmer and Firestone are surely too sweeping and perhaps largely unwarranted. The method is not universally applicable to studies of aural harmonics, but it may be regarded as a valuable experimental technique; and through its use, fairly reliable information should be obtainable on the amount and kind of distortion in the human ear.

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